Vol. 45, No. 4 July, 1938

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

EDITED BY

HERBERT S. LANGFELD

PRINCETON UNIVERSITY

CONTENTS

The Goal-Gradient Hypothesis Applied to Some 'Field-Force' Problems in the Behavior of Young Children:
CLARK L. HULL 271

Determinants of the Perceived Vertical and Horizontal;
JAMES J. GIBSON AND O. H. MOWRER 300

The Postural Substrate:
G. L. FREEMAN 324

Perception and Intelligence: Their Phylogenetic Relation:
NORMAN M. LOCKE 335

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE
PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND OHIO STATE UNIVERSITY, COLUMBUS, OHIO

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS OF

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

PSYCHOLOGICAL REVIEW

HERBERT S. LANGFELD, *Editor*
Princeton University

Contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.
Subscription: $5.50 (Foreign, $5.75). Single copies, $1.00.

PSYCHOLOGICAL BULLETIN

JOHN A. MCGEOCH, *Editor*
Wesleyan University

Contains critical reviews of books and articles, psychological news and notes, university notices, and announcements. Appears monthly (10 issues), the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.
Subscription: $6.00 (Foreign, $6.25). Single copies, 60c.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

S. W. FERNBERGER, *Editor*
University of Pennsylvania

Contains original contributions of an experimental character. Appears monthly (since January, 1937), two volumes per year, each volume of six numbers containing about 625 pages.
Subscription: $14.00 ($7.00 per volume; Foreign, $7.25). Single copies, $1.25.

PSYCHOLOGICAL ABSTRACTS

WALTER S. HUNTER, *Editor*
Brown University

Appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.
Subscription: $7.00 (Foreign, $7.25). Single copies, 75c.

PSYCHOLOGICAL MONOGRAPHS

JOHN F. DASHIELL, *Editor*
University of North Carolina

Consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.
Subscription: $6.00 per volume (Foreign, $6.30).

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

GORDON W. ALLPORT, *Editor*
Harvard University

Appears quarterly, January, April, July, October, the four numbers comprising a volume of 560 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.
Subscription: $5.00 (Foreign, $5.25). Single copies, $1.50.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp. (2 vols.): $17.00 (Foreign, $17.75).
Bulletin and J. Exp. (2 vols.): $18.00 (Foreign, $18.75).
Review, Bulletin, and J. Exp. (2 vols.): $22.00 (Foreign, $23.00).

Subscriptions, orders, and business communications should be sent to

THE PSYCHOLOGICAL REVIEW COMPANY

THE OHIO STATE UNIVERSITY, COLUMBUS, OHIO

VOL. 45, NO. 4 JULY, 1938

THE PSYCHOLOGICAL REVIEW

THE GOAL-GRADIENT HYPOTHESIS APPLIED TO SOME 'FIELD-FORCE' PROBLEMS IN THE BEHAVIOR OF YOUNG CHILDREN[1]

BY CLARK L. HULL

Yale University

The hypothesis of the goal-excitatory gradient (**8**),[2] as well as that of the habit-family hierarchy (**9**),[3] was originally expounded in connection with a consideration of the behavior of albino rats in the ordinary enclosed maze. In such mazes the possible pathways of approach to the goal as well as the range of vision are greatly limited. Indeed, in former papers the sense of vision as a distance receptor was almost entirely neglected. The present paper, on the other hand, is concerned with some of the implications of the two hypotheses and a number of closely related principles when behavior, chiefly locomotor, is taking place in situations such that the field of vision is practically unobstructed. And, while certain limita-

[1] The substance of this article has been in occasional use in the form of a bound manuscript as a seminar reference since the spring of 1934. In this way a number of valuable criticisms have been received; the author is especially indebted to Dr. Neal E. Miller and to Dr. Carl I. Hovland. Mr. Donald T. Perkins has contributed the mathematical portions of notes 6, 12, 16, and 19.

[2] A certain amount of misunderstanding concerning the goal-gradient hypothesis has arisen from confusing one of the implications of the hypothesis under particular conditions (**20**, 153) with the hypothesis itself. The hypothesis, as originally stated (**8**, 26) is, "that the goal reaction gets conditioned the most strongly to the stimuli preceding it, and the other reactions of the behavior sequence get conditioned to their stimuli progressively weaker as they are more remote (in time or space) from the goal reaction." The 'goal' is the point of reinforcement. Because of the somewhat anthropomorphic connotation of the term 'goal' it would, perhaps, be better if this principle could be known as the 'gradient of reinforcement," as suggested by Miller and Miles (**16**).

[3] A habit-family hierarchy consists of a number of habitual alternative behavior sequences having in common the initial stimulus situation and the final reinforcing state of affairs (**11**, 16).

tions will be placed on the opportunities for locomotion in the situations to be considered, freedom in this respect will be comparatively unrestricted. Lastly, the phenomenon to be studied will be mainly the naïve striving behavior of organisms such as young children who are only slightly sophisticated regarding obstacles at distances greater than the arm's length.[4] A number of the problems to be examined have already been subjected to a Gestalt analysis by Lewin (13, 14).

I

We shall begin by considering the influence of the subject's being able to see clearly, though usually at some distance, the lure or goal (*G*) from the starting point of his sequence of locomotion (*S*), together with numerous discriminable aspects of the intervening space. It is clear that the visual stimulation changes constantly, particularly as to the size of the image on the retina, as the subject approaches the lure, *i.e.*, the point of reinforcement. According to the goal excitatory gradient hypothesis, the several phases of the flux of this stimulus complex will become conditioned to the accompanying movements leading to the point of reinforcement with a strength increasing with proximity to the goal. Thus, after a certain amount of training the organism should advance more vigorously the closer it is to the goal (10).[5]

[4] Striving is that behavior of organisms which, upon frustration, displays varied alternative action sequences, all directed by an intent (fractional anticipatory goal reaction or r_G) to the attainment of the same reinforcing state of affairs (11, 16). The naïveté here assumed involves as a minimum the absence of effective speech symbolism. Accordingly the present analysis should apply both to young children and to feebleminded, of whatever chronological age, who have a mental age under about two years (17).

[5] This is on the assumption that the stronger the excitatory tendency, the stronger and more rapid will be the reaction dependent upon it. The experimental results reported by Bruce (3), and one or two other studies which have been communicated privately, seem to indicate that the relationship between the speed-of-locomotion gradient and the hypothesis of the goal gradient may not be so simple as appeared in the author's study just cited and in an independent confirming experiment by Miller and Miles (16). Evidently further experimental work will be needed to clarify this relationship. On the other hand, a recent experiment involving compound trial-and-error, reported by Muenzinger, Dove, and Bernstone (18), presents reassuring evidence of the substantial truth of the basic goal-excitatory-gradient hypothesis (see note 2 above). Incidentally, these writers show that when conditions permit, a similar

Yoshioka's experiment involving the power of rats to discriminate short from long paths indicates that this increase in excitatory tendency follows rather closely the logarithmic principle (22).[6]

A convenient presentation of such a logarithmic function is shown in Table 1. By means of this table it will be easy to see in a clear and precise manner the quantitative [7] implications of the goal-gradient hypothesis as bearing on the locomotor excitatory tendencies of a considerable variety of situations. Let us take as our first example one in which the visual stimulus of the lure (G) is placed at two distances from the subject, the second distance being three times as great as the first (Fig. 1). Suppose that the first starting point (S) is 5 units distant from the goal, and that the second is 15 units distant.[8] By Table 1, the excitatory potentiality of

gradient of excitation also *follows* the point of reinforcement quite in harmony with the well-established conditioning or learning experiments of Thorndike (21; 2).

[6] This may be demonstrated as follows. Yoshioka's experiment showed that the length-discrimination threshold in rats for pairs of distinct paths to the same reward point was inversely proportional to the distance. If we assume that the *difference* between the excitatory tendencies (E) leading to the initial acts of the alternative pathways is the same for the various length-discrimination thresholds, then the decrease in excitatory strength per unit increase in distance (D) from the goal is inversely proportional to the distance, *i.e.*,

$$\frac{dE}{dD} = -\frac{b}{D},$$

where b is a positive constant. Hence, integrating, we have:

$$E = a - b \log D,$$

where a is a constant (and must be positive, since E is positive). This is the equation upon which Table 1 is constructed. It is to be noted, however, that this is but a tentative, and probably not a very accurate, representation of the exact nature of the relationship. Two considerations argue against its finality. (1) At $D = 0$, E becomes infinite, and (2) with very large values of D, E becomes negative instead of becoming asymptotic to zero. A plausible alternative equation is:

$$E = ae^{-kD}$$

In this connection see Guilford (6).

[7] This quantitative approach with the attendant possibilities of utilizing the potentialities of a metricized mathematics for the purpose of exploring the implications of its postulates is somewhat in contrast to the topological approach emphasized by Lewin with its seeming limitation to the qualitative (14).

[8] It is to be emphasized that the principles of alternative reaction sequences here considered are intended to apply to all sorts of behavior and are definitely not restricted

TABLE 1

This table shows the hypothetical strength of the conditioning of stimuli received at different distances from the goal to reactions occurring simultaneously. This is on the assumption that the excitatory tendency one unit from the goal acquires a strength of ten units and that the strength of the excitatory tendency acquired at each of the remaining distances diminishes with their remoteness according to the equation

$$E = a - b \log D,$$

where a has a value of 10, b has a value of 4, and the logarithms are taken with a base of 5.

Units Distant from Goal (D)	Strength of Excitatory Tendency (E)	Units Distant from Goal (D)	Strength of Excitatory Tendency (E)
1	10.000	26	1.903
2	8.277	27	1.809
3	7.270	28	1.718
4	6.554	29	1.631
5	6.000	30	1.547
6	5.547	31	1.465
7	5.164	32	1.386
8	4.831	33	1.310
9	4.539	34	1.236
10	4.277	35	1.164
11	4.040	36	1.094
12	3.824	37	1.026
13	3.625	38	.959
14	3.441	39	.895
15	3.270	40	.832
16	3.109	41	.771
17	2.958	42	.711
18	2.816	43	.652
19	2.682	44	.595
20	2.554	45	.539
21	2.433	46	.484
22	2.317	47	.431
23	2.207	48	.379
24	2.101	49	.327
25	2.000	50	.277

the visual image encountered at 5 units distant has a value of 6.00 points, whereas its strength at 15 units is only 3.27 points, a difference of 2.73. This fundamental relationship may be represented graphically as in Fig. 2.

It follows from the preceding that if an experimental situation should be so arranged that two distinct alternative paths of these respective lengths *both* lead from S to G, path

to locomotion in space. When non-spatial sequences are involved, the distance from the goal should be thought of as time (8) or, possibly, energy consumption. Spatial examples have certain advantages for exposition because of the ease of diagrammatic representation, but it is believed that they actually present greater theoretical difficulties than do non-spatial sequences.

A would be chosen in preference to path *B* because the stimulus at the point of choice would have an excitatory tendency leading to the acts constituting *A* nearly twice as great as to those constituting *B*. A situation of this kind is, of course, the limiting case of a habit-family hierarchy: the fact that two paths begin and end at the same points makes them a family; the fact that one is preferred above the other constitutes them a hierarchy (**9**).

By a simple extension of the above reasoning, certain corollaries may be derived. It may be shown, for example, that the difference in excitatory tendency leading to the locomotor action of traversing pairs of paths differing in length by a constant amount grows less and less as the paths

FIG. 1. Two paths to the same goal; *S*, starting point of one path and *S'*, of the other. Path *A* is represented as being 5 units in length and path *B* as 15 units in length. The dynamics of the two situations are represented in Fig. 2. It is to be observed that if Path $S' \rightarrow G'$ were suitably curved and shifted about in a manner such that point *S'* would coincide with point *S*, and point *G'* would coincide with point *G*, the combination of paths thus resulting would constitute a habit-family hierarchy (see text, pp. 274–275).

jointly become longer and longer. Suppose, for example, that the shorter path in the above example increases from 5 to 20 units in length and the longer one from 15 to 30. The difference in excitatory strength produced by the difference of 10 units in length would (Table 1) shrink from 2.73 points to 1.00 point. When the shorter path is further increased to 40 units in length, a second path 10 units longer makes a difference in excitatory tendency of only .56 of a point, and so on. Conversely, by shortening both paths a constant difference in their lengths will give rise to greater and greater differences in their excitatory potentialities. This principle has important implications which will be taken up below. So far as the author is aware, these corollaries have not been subjected to experimental test; they offer a ready means of further testing the hypothesis.

II

At this point we may conveniently consider the bearing of the present system of hypotheses on the interesting problem of the allegedly deleterious influence of increasing the excita-

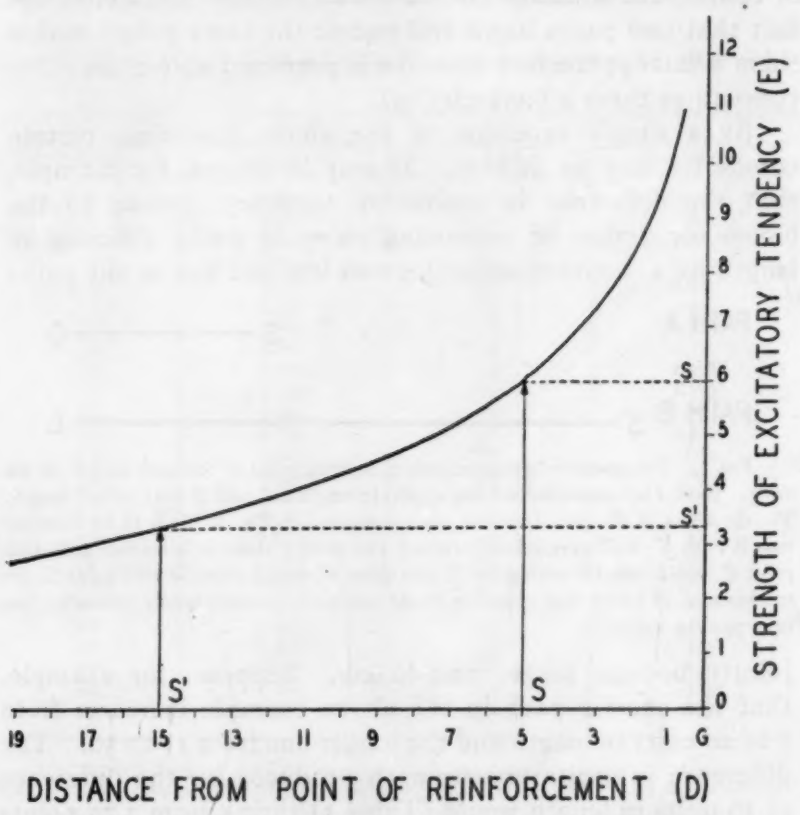

FIG. 2. Diagrammatic representation of the hypothetical dynamics of the two paths to goal *G* shown in Fig. 1. The curve represents the excitatory potentialities of the visual stimulus of the goal complex at all distances from 1 to 19 units according to the equation,

$$E = a - b \log D.$$

The strengths at 5 and 15 units distant have been projected upon the scale at the right. It is evident to inspection that *S* has nearly twice as great excitatory potentiality as has *S'*.

tory strength of the lure upon the solution of certain types of problems (**12**). Let us assume once more that a naïve organism has two distinct habit sequences corresponding to

paths *A* and *B* of Fig. 1, each leading to the same goal or point of reinforcement. In this case, however, there is supposed to be an effective physical barrier at the beginning

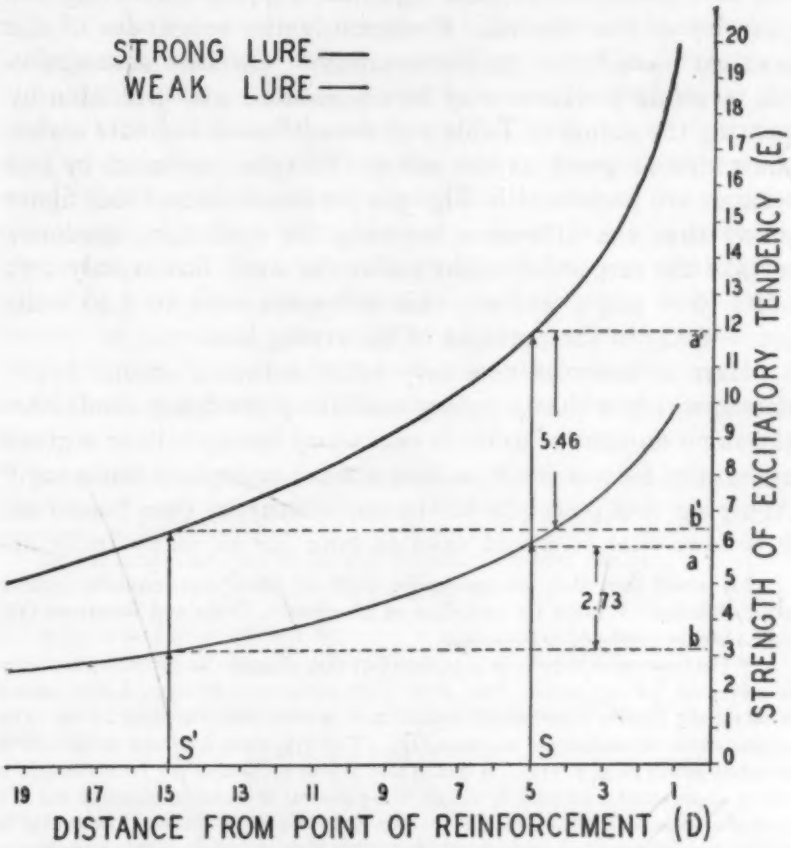

FIG. 3. Diagrammatic representation of the dynamics of doubling the strength of the lure at 1 unit from *G*, Fig. 1. The two curves represent the hypothetical strength of excitatory tendency of the optical image of the respective lures for all distances between 1 and 19 units of remoteness from *G*. The equation for the weak lure (lower curve) is

$$E_w = 10 - 4 \log_8 D,$$

whereas that for the strong lure (upper curve) is

$$E_s = 20 - 8 \log_8 D.$$

The excitatory potentialities (*E*) at *S* and *S'* are projected upon the scale at the right. It is evident to inspection that the difference between *S* and *S'* is greatly increased by adding to the strength of the lure.

of the shorter path (*A*) so that solution of the problem consists in traversing the longer path (*B*). Let us assume, further, that in one situation the excitatory potentialities of the goal one unit distant have been doubled, say, by increasing the quantity of the reward. Presumably the remainder of the gradient would rise proportionately.[9] On this assumption the resulting situation may be represented with precision by plotting the values of Table 1 on two different ordinate scales, one twice as great as the other. Graphs produced in this manner are presented in Fig. 3. An inspection of this figure shows that the difference between the excitatory tendency to take the respective paths under the weak lure is only 2.73 units (6 − 3.27), whereas this difference rises to 5.46 units (12 − 6.54) in the presence of the strong lure.

Here it becomes necessary to introduce a second hypothesis, which is that a strong excitatory tendency diminishes the same number of units in excitatory strength from a given amount of frustration [10] as does a weak excitatory tendency.[11] Applying this principle to the supposititious case before us, it follows that it would take as long (or as many futile at-

[9] It would seem that this assumption could be tested experimentally without great difficulty. Perhaps the technique of Muenzinger, Dove, and Bernstone (18) might here be employed to advantage.

[10] The frustration hypothesis is to the effect that whenever an excitatory tendency is prevented, for any reason, from evoking its accustomed reaction, a state ensues substantially like the experimental extinction or internal inhibition long known to be characteristic of conditioned reactions (19). This hypothesis has been elaborated to a certain extent in (9, p. 139). A special case of wide occurrence not yet elaborated is where an organism is learning by simple trial-and-error to choose consistently one of a pair of alternative reactions such as the two turns of a single *T* maze. According to the frustration hypothesis, whichever alternative is chosen, the excitatory tendencies otherwise leading to the other must be frustrated and so generate a certain amount of inhibition. This would be true even though the alternative chosen should be correct and receive reinforcement. According to this hypothesis, a constant number of reinforcements to one of two alternative reactions, with the stimulus otherwise evoking the other reaction present, should add less to the extinctive resistance of the reinforced reaction than they would if the stimulus normally evoking the weaker alternative were absent. This deduction suggests an experimental test of the hypothesis.

[11] No really satisfactory evidence on this point is available. The present situation makes the experimental determination of this relationship especially urgent. Actually, the following deductions would still hold even if the absolute diminution in excitatory tendency should be faster in initially strong than in initially weak excitatory tendencies, provided that the relative *rate* in the former were slower.

tempts on the part of the organism) for the presence of the barrier to path *A* to diminish by 4 units an excitatory tendency initially with a strength of 12 units (strong lure) as one initially with a strength of only 6 units (weak lure). In the former case the problem would not have been solved in this length of time, since the obstructed path would still have a strength of 8 units (12 − 4) which would be well above the strength of the long but unobstructed path (6.54). In the case of the weak lure, on the other hand, the obstructed path would have, after the same period (or amount) of frustration, a strength of only 2 units (6 − 4). Since this is well below the strength of the long path (3.27) the latter would presumably have been chosen some time before the end of the period in question, solving the problem of the organism. Thus the paradox of an increased lure interfering with, rather than facilitating, the solution of certain problems apparently finds an explanation.[12]

[12] The preceding demonstration holds, of course, only for the particular supposititious values employed. The general theorem may be derived by ordinary mathematical procedures, as follows:

Let the initial excitatory tendency in the case of the weak lure be:

$$E_w = a - b \log D$$

and in the case of the strong lure be:

$$E_s = K(a - b \log D) \text{ where } K > 1.$$

Then, after a certain number (n) of frustrations, these excitatory tendencies will have reduced to:

$$E_w(n) = a - b \log D - f(n),$$
$$E_s(n) = K(a - b \log D) - f(n),$$

where $f(n)$ is an increasing function of n.

Now let path *A*, of length D_1, be obstructed; and let path *B*, of length $D_2 (D_2 > D_1)$, be unobstructed. Then, the number (n_w) of frustrations required to make the excitatory tendency along *A* less than that along *B* is:

$$n_w = [f^{-1}\left(b \log \frac{D_2}{D_1}\right) + 1],$$

where f^{-1} is the inverse function of f, and the brackets mean 'largest integer not greater than.'

Likewise, the corresponding number (n_s) of frustrations required in the case of the strong lure is:

$$n_s = [f^{-1}\left(Kb \log \frac{D_2}{D_1}\right) + 1],$$

but

$$K > 1,$$

In this same general category probably will be found to fall phenomena resulting from increasing the craving of the organism. If the lure is food, general observation as well as experimental results (**10**) indicates that increased hunger (food privation) heightens the excitatory gradient very much as does increasing the quantity of the lure. By the same reasoning as that of the immediately preceding paragraph, increasing hunger with a constant food lure should impede the solution of this type of problem very much as would increasing the lure with constant craving or drive.

Another situation closely paralleling the influence of increasing the excitatory value of the lure while leaving the lure itself objectively unchanged ought, according to the present set of hypotheses, to result from progressively shortening both paths at the same time keeping the absolute difference between them the same. A special case of this kind studied by Lewin (**13, 14**) is presented by the two situations shown in Fig. 4. Suppose that direct path *A* is approximately 5 units long. Then indirect path *B* will be about 29 units long. By Table 1 (leaving out of consideration for the moment the directional factor), the excitatory tendency at point *S* to take these respective paths should be 6.00 units and 1.63 units respectively; this leaves a difference of 4.37 points of excitatory potentiality to be extinguished by the barrier in path *A* before path *B* can become active. In the case of the right-hand figure, direct path *A'* has a length of 12 units and indirect path *B'* has a length of approximately 35

---

hence

$$Kb \log \frac{D_2}{D_1} > b \log \frac{D_2}{D_1},$$

hence

$$f^{-1}\left(Kb \log \frac{D_2}{D_1}\right) + 1 > f^{-1}\left(b \log \frac{D_2}{D_1}\right) + 1,$$

hence

$$n_s \geq n_w,$$

i.e., the number of frustrations on the shorter path necessary to produce the successful (long-path) reaction will be greater with the strong than with the weak lure provided the difference between the lengths of the two paths is great enough to be equivalent to at least a single attempt. The outcome of the two remaining cases in this section may be derived in an analogous manner, but space in which to do this is not available in the present paper.

units. By Table 1, the excitatory tendency at choice point *S* to take paths *A'* and *B'* to *G'* should be 3.82 units and 1.16 units respectively, with a difference of only 2.66 units to be worn down by the barrier frustration before path *B'* can become active. Assuming, again, something like a constant rate of extinction from the frustration produced by the barriers to the direct paths, goal *G'* should be easier of attainment than goal *G*, in some inverse function of the proportion of 4.37 to 2.66.

To sum up, then, we arrive at the tentative conclusion that three different changes in a problem situation are substantially equivalent in their supposed paradoxical ten-

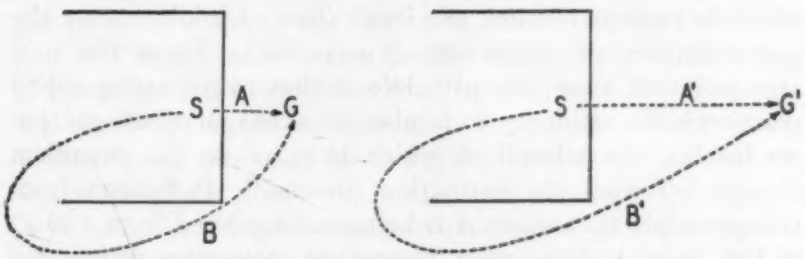

FIG. 4. The figures represent an organism located at *S* and observing a lure *G* and *G'* respectively. There are two potential paths to *G*, *A* and *B*, and two to *G'*, *A'* and *B'*. The problem is which goal will be more easily attained, and why. The present system of hypotheses favor *G'*, providing no other lures complicate the situation by leading the organism around the barrier.

dency to impede the solution of problems involving alternative paths to a goal where the shorter, or preferred, path has a barrier: increasing the strength of the lure, increasing the strength of the drive or craving, and decreasing the remoteness of the lure. The last two of the three cases are capable of general demonstration by the ordinary mathematical procedures analogous to those applied to the first (see note 12). This analysis serves to raise not only the experimental question as to the postulates involved, such as the shape of the goal excitatory gradient and the rate of weakening of excitatory tendency under frustration, but, quite as insistently, the question of the reality in detail of the three supposed paradoxical tendencies themselves. It is highly doubtful whether

either set of phenomena is as simple as the above treatment would imply.

III

Consider once more the situation represented in Figs. 1, 2, 3 and 4, on the former assumption that the short path *A* has a barrier near its beginning. In the preceding sections we have proceeded on the tacit assumption that the only thing necessary before path *B* will be chosen is that the strength of the tendency to take path *A* shall be weakened from s (Fig. 2) to a value somewhat below s'.[13] It now becomes necessary to point out in the competitional situations considered, the operation of a factor—temporarily ignored—which is variously called the irradiation of inhibition or the generalization of extinction (frustration). From the present point of view this principle is that every extinguished reaction has a tendency to inhibit (weaken) all other reaction tendencies, the stimuli of which impinge on the organism closely following the extinction process.[14] It follows from this principle that when *A* is being extinguished from s to s', *B* has been undergoing a secondary extinction which will depress its excitatory potentiality appreciably below s', say to s''. As a result the extinction of *A* must go on below s' to s''. But by this time path *B* will have suffered further secondary weakening.

This raises the question as to how far the extinction of *A* must go before it will actually reach a level beneath that of *B*. So far as the present writer is aware, this problem has not

[13] It is believed that the perseveration principle will insure that the effect of trials on *A* will actually persist somewhat beyond the point at which the strength of *A* has become equal to that of *B*, which would bring it about that *B* would have an appreciably higher excitatory potentiality than *A* when the first trial on the former would take place. Otherwise spontaneous recovery of *A* would instantly recall the subject to that reaction again, and the organism would vibrate frantically from one to the other like too sensitive thermostats which sometimes oscillate continuously between turning the heat on and off.

[14] That this principle is operative in the free and dynamic behavior of rats in the simple trial-and-error situation has been abundantly demonstrated in unpublished work of R. E. P. Youtz (23) and of D. G. Ellson (4). Mitrano (17) has shown it to be operative with feeble-minded children. In the situations so far investigated the generalized weakening effect seems to be around 50 per cent.

yet been investigated experimentally, though it should be a relatively straightforward matter on the conditioned-reflex level. In advance of the possession of actual knowledge of this relationship, however, it is easy to tell roughly what should result under various suppositions. It is clear, for example, that, other things equal, the shorter the distance between s and s', the less inhibition there will be to irradiate and the shorter the distance below s' at which reaction B will take place. Secondly, the smaller the proportion of inhibition which irradiates, the shorter will be the distance below s' at which B will take place.

There remains to be considered the special case of the possibility that A could be extinguished to a functional zero, yet reaction B never take place at all. It seems inevitable that this would occur under certain conditions: Suppose that 50 per cent of the inhibition of A irradiates to B and that at the outset A stands at 6.00 and B at 2.00. When A has reached zero, B would have suffered 3 units of inhibition; this would place it at $-$ 1.00, *i.e.*, one unit below zero (19). In this case striving for this particular goal (reinforcing state of affairs) would cease altogether, at least for a time. If the time available for solution is limited it follows that the organism may completely fail to solve its problem.

IV

At this point we must return to the consideration of a principle which was latent in the situation considered in Section II above, especially as represented in Fig. 4. This is the influence (in the case of very naïve subjects) of the angle which any path except the one leading directly to the goal makes with the latter. The consideration of this problem may be facilitated by referring to the diagram shown as Fig. 5. The question is: What bearing does the size of the angles of paths B, C, D, E, and F with path A at point S have on the relative strengths of excitatory tendency of the paths in question?

The hypothesis here tentatively put forward is that in the random locomotion, and even hand movements, of the

organism (7) [15] throughout its previous existence, a movement terminating at *G* but initiated in the direction of path *B* at point *S* will, *on the average*, require the traversing of a distance something like that of path *B*. The same is assumed of the initial angles of paths *C*, *D*, *E*, and *F*. As a result, the principle of the goal excitatory gradient demands that the greater the angle of the path at point *S*, the weaker will be

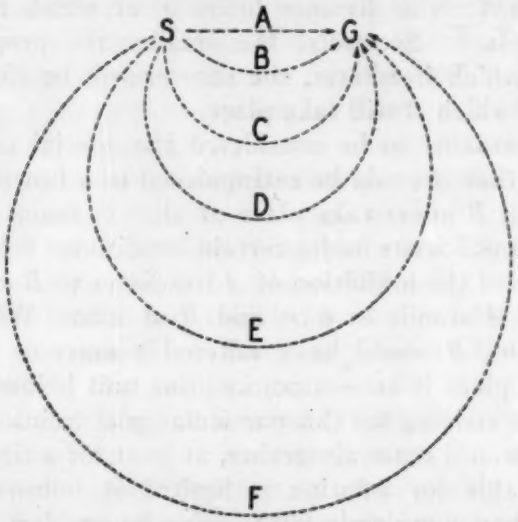

FIG. 5. Diagrammatic representation of what are conceived to be approximations to typical *mean* pathways by which the organism has passed by means of random locomotion in its previous history from one point (*S*) to another point (*G*). It will be noted that the angle which each path makes with path *A* at point *S* is a direct function of the length of the path—the larger the angle, the longer the path. It is understood that a substantially similar family of potential circular paths lies on the other side of direct path *A*.

the tendency to take the particular path. Thus path *F* as represented in Fig. 5 is about eight times as long as path *A*. Assuming path *A* to be 5 units in length, and path *F* to be 40, the respective excitatory tendencies will be, according to Table 1, in the proportion of 6.00 to .83; *i.e.*, the strength of the tendency to start for *G* in the initial direction of path *A*

[15] Halverson's analysis of hand and arm movements in the act of prehension among young children (7, 175) shows that the pathway of the arm and hand in reaching for a small cube at 28 weeks is a decidedly circuitous one but, with increasing age (52 weeks) its course becomes practically a straight line.

would be over seven times as great as the tendency to set out in the initial direction of path *F*. This hierarchy of excitatory tendencies based on the initial angles of locomotion toward a goal thus appears to be a special case of the habit-family hierarchy (9).

It follows at once from these considerations that in certain situations the difference between the excitatory tendencies of a naïve organism to take the initial steps of two paths may

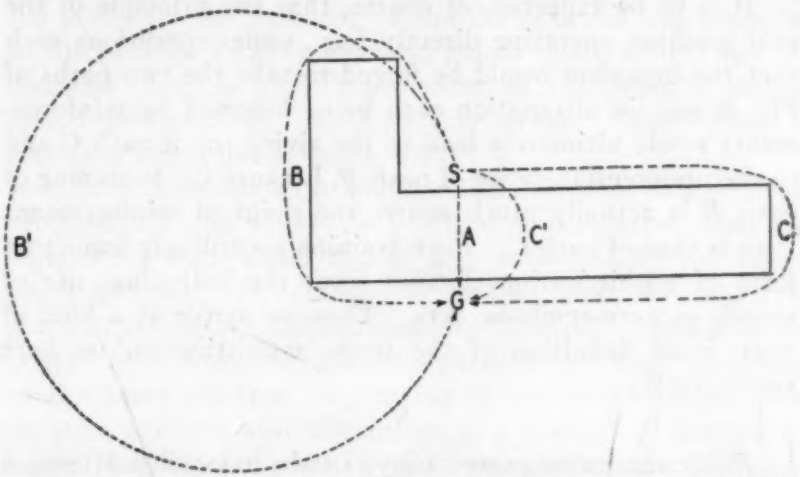

FIG. 6. Diagrammatic representation of an experimental situation designed to show the influence of the angle of the initial portion of a path upon the strength of its tendency. *S* is the organism, *G* is the lure. The right-angled figure in bold outline represents a barrier. Path *B* is actually shorter than path *C*, yet it is assumed that the naïve organism will choose *C* rather than *B* because at the outset *C* is the beginning of paths which, on the average, in the past have been approximately of the length of path *C'*, and path *B* at the outset is the same as paths which in the past have averaged in length something like that of *B'*.

be considerably greater than the difference between the actual lengths of the two paths would warrant. This may be illustrated by the situation represented in Fig. 6. Let us suppose that the organism at *S* has the choice of paths *B* and *C* around the barrier to *G*. Path *C* is 26 units long, whereas path *B* is only 22 units long. Nevertheless, a naïve organism might be expected to choose path *C* because the initial angle of path *C* has presumably in its past history represented, on the average, a path something like *C'*, of only 8 units, whereas the

initial angle of path *B* has, on the average, represented a path something like that of *B'*, which has a length of 42 units. By Table 1, the angles therefore represent naïve excitatory tendencies in the proportion of 4.8 to .7 in favor of path *C*, *i.e.*, from the point of view of the initial angle of the pathway the excitatory tendency to take path *C* should be something like seven times as great as that to take path *B*, even though path *B* is actually the shorter of the two.

It is to be expected, of course, that the principle of the goal gradient operating directly (*e.g.*, under conditions such that the organism would be forced to take the two paths of Fig. 6, say, in alternation each being followed by reinforcement) would ultimately lead to the giving up of path *C* and to the preferential choice of path *B*, because the beginning of path *B* is actually much nearer the point of reinforcement than is that of path *C*. Such training accordingly leads to a form of sophistication distinct from the individual use of speech as pure-symbolic acts. Thus we arrive at a kind of operational definition of the term 'sophistication' as here employed.[16]

V

With admirable perspicacity, Lewin has called attention to a very important series of problems involving conflicting tendencies to action, *i.e.*, situations which involve simultaneous impulses both to approach and to retreat. Such conflicts of excitatory tendency he has aptly termed *tensions* (13). One supposititious case illustrating this point is that of a child

[16] It is possible to state the angle-hierarchy hypothesis (Fig. 5) in exact mathematical terms. On this hypothesis the distance (D) as a function of the angle made by the tangent of the arc at point S with the straight line joining S and G (the distance SG being taken as the unit of measurement) is given by the equation,

$$D = \frac{A}{\sin A}.$$

Since the excitatory tendency (E) at the beginning of a pathway on the goal-gradient hypothesis is a function of D, E may be expressed in terms of the same variables by the equation,

$$E = a + b \log \sin A - b \log A.$$

This, of course, can be regarded as no more than a very rough first approximation to the actual relationship. It is hoped, however, that it may serve as an entering wedge to an investigation of the whole problem. It may at least claim the virtue of being sufficiently definite to be susceptible of experimental verification.

on the shore looking at a toy swan floating in the water. The swan as a stimulus object tends to evoke in the child movements of approach, whereas the water as a stimulus object tends to evoke movements of flight. In this connection Lewin remarks (13, p. 607), "It is important that here, as frequently in such cases, the *strength* of the field forces which correspond to the negative valence diminishes much more rapidly with increasing spatial *distance* than do the field forces corresponding to the positive valence. From the direction and strength of the field forces at the various points of the field it can be deduced that the child must move to the point *P* where *equilibrium* occurs. (At all other points there exists a resultant which finally leads to *P*.)" [17]

A still more striking and dramatic form of this general problem is found where the positive and negative stimulus objects are both small and occupy substantially the same point in space. It is assumed, further, that the negative gradient at one unit distant from the object, say, has a greater excitatory potentiality than the positive excitatory gradient at the same distance but, owing to its steeper slope, the negative gradient soon diminishes to a strength of excitation below that of the positive gradient, thereafter remaining permanently in an inferior position. As pointed out above, there is evidence indicating that the positive excitatory gradient possesses a roughly logarithmic shape. Corresponding study of negative excitatory tendencies has not yet been made. Analogy, however, suggests that negative excitatory tendencies may show at least a negative acceleration.[18]

[17] It is to be regretted that Lewin did not work out in detail the deduction which he mentions as a possibility. The above quotation suggests that had this been done his assumptions would turn out to be substantially the same as the relevant assumptions of the present study (see summary) and the form would not be very different from that represented by note 19, below. It is believed that if such practices could become general the genuine disagreements among the psychologies would prove to be much less than now appears from the divergence of their vocabularies.

[18] Here evidently lies a virgin field for investigations both theoretical and experimental. Will, for example, a negative gradient lead to the elimination of blinds in a maze? If so, will the order be backward or forward or in some more complex mode? Ordinary observation suggests the probability that suitable measurements would readily reveal a marked speed-of-locomotion gradient (10); *i.e.*, that the organism would proceed more slowly the more remote it is from the point of retreat.

A situation based on these assumptions is represented diagrammatically in Fig. 7. The positive gradient JJ'

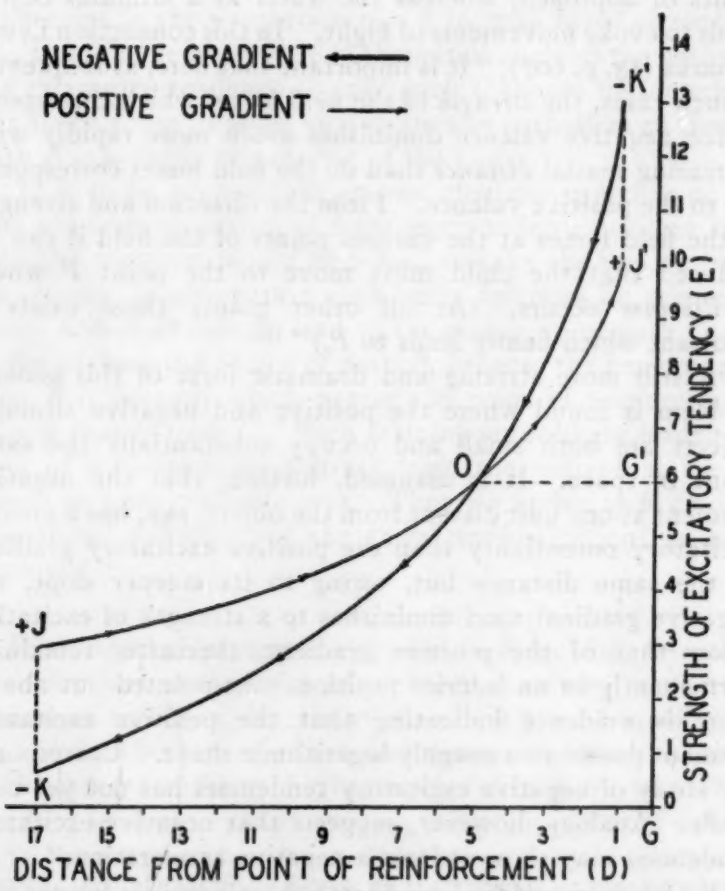

FIG. 7. Diagrammatic representation of the functional relationships of two gradients of different sign and different slope. The positive gradient is represented by the equation,

$$E_+ = 10 - 4 \log D,$$

and the negative gradient is drawn according to the equation,

$$E_- = 13 - 7 \log D.$$

The two gradients intersect at $D = 5$ with a common E value of 6 (see note 18).

represents the excitatory potentialities of G as a stimulus to distance receptors leading to locomotor reactions of approach,

whereas the negative gradient KK' represents the excitatory potentialities of G at the various distances to evoke movements of retreat or flight. It may be seen in the figure that at a distance of 17 units the positive gradient dominates; accordingly the organism should move forward with a force corresponding to the difference in excitatory tendency between J and K. However, as locomotion continues it should diminish in speed progressively until point O is reached, at which forward locomotion should cease altogether except for momentum effects. If, on the other hand, the organism should find itself at a distance of one unit from G, locomotion would take place in an opposite direction. The rate of locomotion in this case, as in the one just considered, would presumably correspond to the difference in excitatory tendency existing between K' and J'. This should diminish progressively as point O is approached, at which it should cease just as when the movement was in a forward direction.[19]

[19] The general case takes the following form:

Let the gradient of the positive excitatory tendency (E_+) be represented by the equation,

$$E_+ = a - b \log_f D,$$

and the equation representing the negative excitatory tendency (E_-) be

$$E_- = c - d \log_f D,$$

where

$$c > a$$

and

$$d > b.$$

The point at which the difference between the two gradients will be zero will be where they intersect, *i.e.*, where

$$E_+ - E_- = 0,$$

i.e.,

$$E_+ = E_-.$$

Then,

$$a - b \log_f D = c - d \log_f D,$$

i.e.,

$$a - c = (b - d) \log_f D,$$

hence

$$\log_f D = \frac{a - c}{b - d},$$

therefore

$$D = f^{\frac{a - c}{b - d}},$$

where f is the base of the system of logarithms employed. This is the equation expressing the point at which tensional equilibrium should occur, *i.e.*, the distance from the

As a corollary from the above deduction it is evident that, assuming radial symmetry of the positive and negative gradients about point *G*, distance *OG* must be constant in all directions. It follows from this that the locus of tensional equilibrium about point *G* must be a circle (Fig. 8). Assuming the presence in the situation of stimuli leading to action in the direction of numerous minor goals located according to

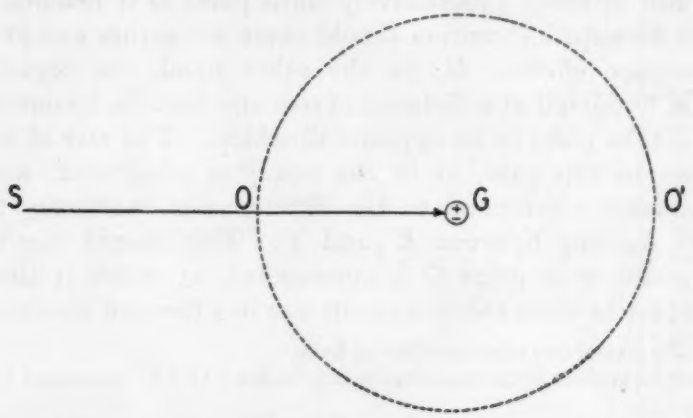

FIG. 8. Diagrammatic representation of the locus of the point of equilibrium when the critical stimulus giving rise to two gradients of opposite sign such as those represented in Fig. 7, emanates from a single point *G*. So long as the gradients remain constant, distance *OG* must remain constant, which necessarily makes the locus of equilibrium circular, as shown by the fine dotted line.

chance, it is to be expected that there will occur irregular oscillations not only toward and away from point *G*, but laterally as well. In the former case it is to be expected that such oscillatory tendencies will be opposed by the summation gradient tending to the locus *O*, whereas those tangent to this locus will meet with no such opposition. It follows that

object precipitating the ambivalent situation at which the organism will come to rest. Thus in the gradients represented in Fig. 7,

$$a = 10,\ b = 4,\ c = 13,\ d = 7,$$

and the base of the log system is 5.

Substituting these values in the above equation, we have

$$D = 5^{\frac{10-13}{4-7}} = 5^{\frac{-3}{-3}} = 5,$$

i.e., the gradients intersect at a distance of 5 units from the goal, as appears in Fig. 7.

the lateral movements will display a greater amplitude than those perpendicular to locus *O*. Accordingly the naïve organism if given time enough may be expected to make completely circular movements about point *G*, though these will be complicated by approaches, retreats, and much retracting.

As a second corollary it should be pointed out that even without the presence of distracting stimuli the organism at point *O* (Fig. 8) probably will not remain absolutely passive, *i.e.*, in a state indistinguishable from mere idleness characteristic of no effective stimulation whatever. On the contrary, it is reasonable to expect that, since there is a great deal more stimulation of a dynamic sort present in conflict situations than in a case where inaction is due to the lack of effective stimulation, the muscles of the organism should show a conspicuous excess of tonicity in the former case as contrasted with the latter. Muscular tonicity may accordingly offer a convenient supplementary means for gauging the extent of this type of conflict.

The deductions of the preceding paragraph tacitly assume a constancy of both the positive and the negative gradients while the organism remains suspended, as it were, between the antagonistic difference gradients to action which the situation evokes within him. There is strong reason to doubt such a constancy. In addition to the very general empirical principle that in organisms tendencies to action are always in a state of flux, there is in this case the specific expectation that, assuming an objectively constant *G* as a stimulus object, both gradients should diminish in height, presumably throughout their whole effective length. This follows from the frustration hypothesis already considered (p. 278).[20]

A test of the above hypothesis which immediately suggests itself could be arranged as follows: Place two objects, one with

[20] There is abundant evidence to support this hypothesis where the frustration is produced by an external barrier of some sort. But the writer is unaware of any evidence bearing directly on situations like the one here under consideration in which the same object gives rise to what is, in some sense, an internal conflict of two gradients of opposite sign. Deductions based on this more inclusive interpretation of the frustrational hypothesis must accordingly be regarded with more than usual caution until definite evidence on the point is available.

a negative value and the other with a positive value, one above the other so as to produce the conflicting gradients shown in Fig. 7. Determine the initial potentialities of each object separately at the outset by measuring the speed-of-locomotion of given organisms in a positive or negative direction at representative distances from *G*. Then place both objects together and let the organism remain at point *O* (Figs. 7 and 8) in the state of tensional equilibrium for about as long as this will persist. At once repeat, with separate subgroups, the test of the excitatory potentiality of each object singly, as at the beginning of the experiment. The frustration hypothesis demands that in this latter case the rate of locomotion toward the positive object and that away from the negative object shall both be diminished.

Two corollaries flow from this deduction. The first, based on the principle of spontaneous recovery (**19**), is that after a lapse of time (possibly within the range of one or two hours) the speed of locomotion in both cases should have increased to a very considerable extent, though the recovery should not be perfect. A second corollary, again based on principles derived from conditioned-reflex experiments, is that an organism placed in the state of tensional equilibrium a second time will show both a more rapid inhibition of the respective excitatory tendencies than it did on the first occasion and also a slower and more imperfect spontaneous recovery.

One of the most obvious questions which arise from the consideration of conflict situation when the latter is viewed in the light of the frustration hypothesis, is the effect of the continuation of the state of tensional equilibrium upon the distance (*OG*) from the goal objects (*G*), which the organism at first maintains. It is obvious, of course, that if both gradients should decline simultaneously and at a certain relative rate the distance *OG* would remain constant. When we consider the number of other possibilities of variation, however, the chance that this particular set of changes should take place is exceedingly improbable. It accordingly seems

almost certain that the distance *OG* will undergo a progressive change in length with the duration of the conflict or tension.

Whether, upon the whole, the change will be to diminish or to increase distance *OG* can only be predicted when we know more concerning the resistance of positive and negative, steep and gentle, gradients. It is evident that here is a rich and almost virgin field for investigation, both with animals and with young children. One apparently relevant principle we have, however, thanks to the work on conditioned reactions. Other things equal, that gradient which is the oldest should resist frustration the best. This factor should tend to lengthen or shorten distance *OG* according to whether the older gradient is the negative or positive one respectively.

In this connection there should also be considered the matter of progressive changes in the drive. As already pointed out above, an increase in a drive such as hunger presumably increases both the height and the slope of the positive gradient. This increase should obviously diminish the distance *OG* and reduce the radius of the circular locus of tensional equilibrium.

Finally, the significance of this whole psychology of conflicting excitatory gradients as a challenge to experimental psychology should be emphasized. Its importance for psychopathology, particularly the conflicts of the positive sexual gradient and the specific negative gradient based on social tabus and prudential considerations, has been emphasized in the clinical field by the psychiatrists for many years (**5**). Coitus interruptus and what, in current slang, is called 'petting,' are cases in point. The bad repute of these practices in mental hygiene emphasizes the possible significance of conflicting gradients in either time or space. In this connection it is to be noted that the mutual checking of two impulses as represented in Fig. 7 might be expected to produce something like twice as great an amount of internal inhibition as would be the case where one tendency was completely over-ridden by the other because in the one case two excitatory tendencies are frustrated, whereas in the other, only

one is thwarted. It would not be surprising if this mechanism should be found to play an important role in the so-called psychogenic disorders.

Summary

The preceding analysis of certain problems in the behavior dynamics of naïve organisms has proceeded mainly on the following assumptions:

1. That simultaneously occurring stimuli and reactions (both of approach and flight) tend to be associated more strongly the closer they are to a reinforcing state of affairs, the diminution in excitatory tendency with remoteness from the point of reinforcement proceeding with a negative acceleration.

2. That situations may arise where stimuli originating in substantially the same point in space will give rise in a given organism to incompatible excitatory tendencies, notably the opposing tendencies to acts of approach and of flight.

3. That the flight gradient may, at least under certain circumstances, be both steeper and higher near the stimulus object.

4. That an organism in which positive and negative excitatory tendencies are active behaves at any given instant according to the algebraic sum of such tendencies.

5. That the positive gradient of excitation grows both higher and steeper with the increase in the drive, *e.g.*, food privation.

6. That, other things equal, this gradient of excitation grows higher and steeper with the increase in the amount of reward.

7. That a strong functional excitatory tendency is weakened after a given amount of frustration by a smaller proportion of its original strength than is a weak functional excitatory tendency.

8. That spontaneous recovery from extinction effects will occur but this will not be complete.

9. That a second extinction will occur more rapidly than the first and its spontaneous recovery will be less complete.

10. That in the history of organisms it is a fact that upon the whole the larger the angle the beginning of a pathway makes with the straight line leading to the object, the longer the path to the object.

11. That the prevention by any circumstance of the reaction normally evokable by any stimulus or stimulus component results, other things equal, in weakening (extinguishing) the particular excitatory potentiality of such stimulus or stimulus component.

12. That the older an excitatory tendency, the more resistant to the extinction from frustration.

13. That a considerable weakening of a given excitatory tendency appears at once following the extinction of another excitatory tendency.

From these assumptions in the main the following conclusions have been drawn concerning the behavior of naïve organisms, unmentioned factors being assumed as equal:

1. In a problem situation such as shown in Fig. 4, a naïve organism will have more difficulty in solving the problem where the lure is large than where it is small.

2. There will be more difficulty where the lure is close to the barrier than where it is farther away.

3. There will be more difficulty where the drive is strong than where it is weak.

4. The functional excitatory tendency of weak alternatives in a habit-family hierarchy may be depressed to 'below zero' by generalized extinction effects from the frustration of a stronger member of the hierarchy.

5. This depression below zero is the more likely to occur, the greater the percentage of the extinction effects in the strong tendency which are generalized to the weak tendency.

6. The depression below zero is more likely to occur the weaker the absolute strength of the weak tendency.

7. The depression below zero is more likely to occur the greater the difference between the competing excitatory tendencies.

8. For a naïve organism, the larger the angle which the beginning of a pathway to a point makes with the straight line

to the point, the weaker the excitatory tendency to execute the acts which constitute taking the divergent path.

9. Naïve organisms through the misleading action of the angular hierarchy will, under certain circumstances, choose the longer of two paths.

10. With sophistication of the organism consisting of actually traversing both of such paths as considered in (9), the tendency to take the longer path will gradually give place to a degree of preference to be expected on the basis of the uncomplicated goal excitatory gradient hypothesis.

11. Under the conditions of assumptions 2 and 3, there will be a distance (*D*) from the ambivalent stimulus object at which the two opposed excitatory tendencies of a tensional situation will be equal.

12. Under the conditions of (11), the organism will tend to move toward this point of equal excitatory tendency.

13. Under the conditions of (11), assuming an unchanging stimulus object and the presence of minor lures of appreciable potentiality distributed in a chance manner throughout the neighborhood, the organism will tend to take a roughly circular course around the ambivalent stimulus object.

14. These circular movements will be very irregular in rate and extent, and be characterized by much retracing.

15. There will be some irregular oscillation toward and away from the ambivalent stimulus.

16. The movements of (15) will, upon the whole, be less in extent than will those of (14).

17. Where cases of tensional equilibrium persist for an appreciable period, both excitatory tendencies arising from an ambivalent stimulus object will be weakened progressively.

18. With the passage of time there will be a progressive recovery from this weakening.

19. Recovery from this frustrational weakening will never be complete.

20. A second period of tensional equilibrium will produce an extinctive weakening more rapid than the first.

21. The second recovery will be less rapid than the first.

22. The second recovery will be less complete than the first.

23. As the state of tensional equilibrium continues there will usually occur a progressive shift in the distance the organism maintains from the stimulus object.

24. This shift will tend to be in the direction of the older of the two opposed excitatory tendencies.

25. The secondary frustration effects will be greater in cases of tensional equilibrium than in cases where one tendency is strong enough to over-ride the other.

Of the forty or so propositions assembled above from the preceding theoretical analysis, scarcely one is yet established on a secure quantitative experimental basis. Perhaps the chief outcome of the analysis is the sharp realization of our profound ignorance concerning the essential principles operative in such relatively simple dynamical situations. However, admissions of ignorance, while momentarily depressing, are likely to be wholesome. Each recognized item of basic ignorance constitutes a challenge to a critical experimental determination. Thus a resolute attempt at theoretical integration naturally leads the way to a systematically coördinated program of investigation. In the present case, such an integrated program of research appears to fall into three portions or phases.

The first phase consists of the direct experimental determination of the principles or laws, now almost entirely in the state of hypothesis or guesswork, which are suggested by the first of the above lists of propositions. These determinations should be quantitative in nature and so designed as to yield functional curves of basic relationships. Equations fitted to such data become the postulates of the system.

The second phase is logical and mathematical. It consists of deriving by means of mathematics, and perhaps of symbolic logic, the behavioral implications of the postulates yielded by the first movement when acting in the greatest possible variety of conditions. The substance of note 12 gives an indication of the general nature of this theoretical portion of the program.

The third phase consists of the systematic experimental verification of the behavioral expectations resulting from the mathematical activities of the second movement. Whenever this systematic verifying procedure reveals disagreements, work on the first movement is resumed in an effort to rectify presumptive defects in the postulate determinations. Following such new postulate determinations, new implications are drawn, new verification experiments are set up, and so on in continuously recurring cycles until disagreements fail to manifest themselves, if such a time ever comes.

Such a self-conscious scientific procedure may with some propriety be called 'logical empiricism.' It begins with an empirical determination of its postulates and ends with an empirical check on the validity of its theorems; between the two lies the integrating symbolic structure of logic and mathematics.

REFERENCES

1. Anderson, A., Runway time and the goal gradient, *J. Exper. Psychol.*, 1933, **16**, 423–428.
2. Brandt, H., The spread of the influence of reward to bonds remote in sequence and time, *Arch. Psychol.*, 1935, **180**, pp. 40, 41.
3. Bruce, R. H., An experimental investigation of the thirst drive in rats, with especial reference to the goal gradient hypothesis, *J. Gen. Psychol.*, 1937, **17**, 49–62.
4. Ellson, D. G., Quantitative studies of the interaction of simple habits; I. Recovery from specific and generalized effects of extinction (to be published).
5. Freud, S., Selected papers on hysteria and other psychoneuroses. (Trans. by A. A. Brice), pp. 141–142, Journal of Nervous and Mental Disease Publishing Company, 1909.
6. Guilford, J. P., A generalized psychophysical law, Psychol. Rev., 1932, **39**, 73–85.
7. Halverson, H. M., An experimental study of prehension in infants by means of systematic cinema records, *Genet. Psychol. Monog.*, 1931, **10**, 107–286.
8. Hull, C. L., The goal gradient hypothesis and maze learning, Psychol. Rev., 1932, **39**, 25–43.
9. ——, The concept of the habit-family hierarchy and maze learning, Psychol. Rev., 1934, **41**, Part I, 33–52; Part II, 134–152.
10. ——, The rat's speed-of-locomotion gradient in the approach to food, *J. Comp. Psychol.*, 1934, **17**, 393–422.
11. ——, Mind, mechanism, and adaptive behavior, Psychol. Rev., 1937, **44**, 1–32.
12. Köhler, W., The mentality of apes, New York: Harcourt, Brace and Co., 1925.
13. Lewin, K., Environmental forces, in 'A Handbook of Child Psychology' (edited by C. Murchison, 2nd edition revised), Worcester: Clark Univ. Press, 1933, Chapter 14, pp. 590–625.

14. ——, A dynamic theory of personality (Trans. by D. K. Adams and K. E. Zener), New York: McGraw-Hill Book Co., 1935, pp. ix + 286.
15. ——, Principles of topological psychology (Trans. by F. and G. M. Heider), New York: McGraw-Hill Book Company, 1936, pp. xv + 231.
16. Miller, N. E. and W. R. Miles, Effect of caffeine on the running speed of hungry, satiated, and frustrated rats, *J. Comp. Psychol.*, 1935, **20**, 397–412.
17. Mitrano, A. J., A preliminary investigation of human motivation by conditioned reaction methods, Ph.D. thesis, 1937, Yale University Library.
18. Muenzinger, K. F., Dove, C. C. and Bernstone, A. H., Serial learning: II. The bi-directional goal gradient in the endless maze, *J. Genet. Psychol.*, 1937, **50**, 229–241.
19. Pavlov, I. P., Conditioned reflexes (Trans. by G. V. Anrep), Oxford: Oxford Univ. Press, 1927.
20. Snygg, D., Mazes in which rats take the longer path to food, *J. Psychol.*, 1936, **1**, 153–166.
21. Thorndike, E. L., An experimental study of rewards, Contributions to Education (see pp. 54–55), No. 580, New York: Bureau of Publications, Columbia University, 1933.
22. Yoshioka, J. G., Weber's law in the discrimination of maze distance by the white rat, *Univ. Calif. Publications in Psychol.*, 1929, **4**, 155–184.
23. Youtz, R. E. P., The trial and error reaction as a type of conditioned response, Ph.D. thesis, 1937, Yale University Library.

[MS. received November 8, 1937]

DETERMINANTS OF THE PERCEIVED VERTICAL AND HORIZONTAL

BY JAMES J. GIBSON, *Smith College*
AND
O. H. MOWRER, *Yale University*

Introduction

The fact that the seen world appears upright is so obvious that it may seem axiomatic. Nevertheless this vertical-horizontal stability of phenomenal things is as much a psychological problem as their coloration, size, or distance. Stratton once asked the question, "why do things look right side up instead of inverted?" But a different and more fundamental question is, "why do things look right side up in the first place—instead of, for instance, tilted?" Some attention has been paid to the related observation that the objective visual field remains phenomenally upright when the *head* is tilted (**23, 30**). But this phenomenon presupposes a more fundamental one, the fact that the objective visual field is phenomenally upright when the head is *not* tilted.

Koffka has recently described phenomenal space not only as a 'ground' for the perception of objects but also as a 'framework' with reference to which they are oriented and localized (**23**). Space, as experienced, has the quality of 'up-downness' and 'right-leftness'; vertical and horizontal reference-axes are an implicit characteristic of all seeing. Or, in other words, we seem to possess a visual 'sense' of the vertical and horizontal. In support of this assertion, innumerable experiments have proved that we can *imagine* these axes in a perfectly homogeneous circular visual field, and can set an adjustable line in this field to the vertical or horizontal with great precision. This perceptual ability coexists with the ability to maintain and sense deviations from an erect posture, and also to feel the normality or tilt of the terrain and other

environmental objects. Presumably these two sets of facts are the perceptual and the behavioral aspects of a common function. But the perceptual fact of a visual 'sense' of the vertical and the behavioral fact of postural orientation are not related in any simple way as cause and effect. There is evidence to show that, to a considerable degree, they are mutually dependant.

Gelb, for example, describes a patient with a brain injury who suffered from "a pathological change in the main directions of space" (15). The visual environment, including houses, trees, and streets, appeared uniformly tilted to the right (8°–12°) and a visual line looked upright (even with totally dark surroundings) only if it were objectively tilted to the left. The significant fact, however, was that the environment felt as well as looked tilted to the right; the patient's posture was objectively inclined to the left and only in this position did he feel himself erect. Gelb concludes that "the fact of pathological change in *both* the visual and the kinaesthetic-motor realms . . . argues an underlying change of which these are symptoms." It is reasonable *a priori* that the visual and the kinæsthetic vertical should have to coincide, for the body after all is usually a visual object having a definite position and orientation in the visual field.

The sensory determinants of visual and postural uprightness have been rather thoroughly explored since the time of Mach. A large number of sensory cues have been shown to operate, divisible into two classes, first those arising from the pull of gravity upon the body and from the maintenance of postural equilibrium (g-factors), and second those arising from purely visual stimulation (v-factors). The first class includes excitations from the semicircular canals, utricle and saccule, and from the compensatory reactions to which they give rise; from bilateral variations in tactual pressure on the soles of the feet, buttocks, elbows, etc.; from the muscle-sense and the resulting tonic reflexes; and probably also from internal visceral pressures. The second class includes the retinal stimulation produced by the dominant lines of objects and surroundings in the visual world, that is to say the

gravitational verticals and horizontals which nature and more especially civilization present to our eyes; and also the stimuli correlated with the *familiar appearance* of shapes not having such dominant lines (**19**); and finally the shape of the boundary of the visual field formed by the nose, cheeks, and the supra-orbital ridges (**7**). The outlines of the nose, for example, are a part of every visual field and provide standard visual lines which might serve to fix axes of reference. These v-factors, it must be remarked, rest on the assumption that a fixed posture is maintained. The point will be expanded later.

It is obvious that these various stimulus-factors must interact in a complex way when one considers how they would be modified, sometimes concomitantly and sometimes not, in different situations. A considerable body of evidence has accumulated, both experimental and observational, bearing on the relative influence of the g-factors and of the v-factors in determining the visual vertical and also the postural vertical. Some of this evidence is conflicting. Koffka has proposed a bold theory to the effect that the phenomenal 'spatial framework' is constituted by the main lines of organization in the visual field; in other words the principal directions seen in this field create the frame of reference for it (**23**, chap. 6). This means of course that if a visual scene is artificially tilted, or even if its main lines (a window frame or the horizon) are inclined, the framework will follow and a new vertical will be established. The theory is consistently phenomenological, for it makes even the position of the body—the 'ego'—dependant on this visual framework. The ego, being only one of the objects in visual space, is assumed to be oriented in the same way as any other object—by reference to the main lines of visual organization.

There are indications to support such a theory of complete visual dominance but on the other hand there are many facts which contradict it. It is our purpose to evaluate some of this conflicting evidence and to propose a hypothesis: *that*

both the visual and the postural vertical are determined by visual factors and gravitational factors acting jointly, with orientation to gravity, however, as the more decisive factor in cases of real conflict between the two types of sensory data, and the primary factor genetically. We shall first examine the evidence for vision, and then the evidence for gravity.

The experiments and observations which we have to examine are of two sorts, first those in which the v-factors are modified or eliminated, as when the visual field is tilted or the subject is blindfolded, respectively, and second those in which the g-factors are modified, as when the pull of gravity is shifted by a centrifugal force.[1] It is important to remember that we are concerned here with the elimination or shift of one kind of determinant relative to the other. There is a class of experiments related to this method and easy to confuse with it which is irrelevant to our particular problem. They have to do with the effect, or more generally the absence of effect, on the visual and kinæsthetic vertical of *abnormal bodily postures, e.g.*, of tilting the head or body. In this situation one kind of determinant is not shifted relative to the other and they do not conflict; instead both are shifted concomitantly and by the same amount. From an objective point of view *neither* the pull of gravity nor the visual scene is shifted but only the subject's body and therefore the way in which these two affect his sense-organs. The seen and felt environment remain phenomenally upright when the head is tilted despite the change in the proximal stimuli; we are dealing, therefore, with a compensatory perceptual process of some sort—a type of constancy-phenomenon. Many experiments have verified the observation that in a dark room with only a single vertical line of light visible this constancy is only partial and the phenomenal direction of the line is a compromise between the objective and the retinal direction, but this fact, the Aubert phenomenon, must

[1] It is clearly impossible to eliminate the gravitational determinants entirely, although Garten (14) has in the absence of vision experimentally isolated several of them from one another.

be regarded as a special case of a much more general tendency.[2]

In this view of the matter, the problem of the constancy of the perceptual framework, seen and felt, is to be distinguished from the prior problem of its sensory determinants and in particular the role played by vision. Koffka's 'visual organization' theory of the framework is eminently capable of explaining constancy, and was designed for this purpose. It remains to be seen whether it will stand up against experimental evidence of a simpler type.

Evidence for the Dependance of the Vertical on Visual Determinants

Elimination of visual cues.—That postural orientation is somewhat weakened in the dark, or when a subject is blindfolded, has been frequently noted On top of high buildings, on mountain sides, or in any unfamiliar homogeneous visual field where the usual lines of 'anchorage' are absent, normal posture is often reported as difficult to maintain, although it is possible that emotional in addition to purely sensory factors are at work here. Airplane pilots flying in clouds are reported to have emerged upside down without having been aware of their altered position. Failing vision is believed to be a frequent cause of the loss of balance in old age (21). This evidence has even been formulated in the statement that "we stand with our eyes." The inference from these observations is that if the postural vertical is so dependant on visual cues, the visual vertical must be even more so. Strictly interpreted, however, these facts demonstrate no more than the occurrence of orientational errors under particular circumstances in the absence of v-factors. They show that the latter are contributing determinants for orientation but not

[2] Investigations in this special field are complicated by the counter-rolling of the eyes in the head, which is however insufficient to account for either complete or partial constancy of the vertical, and by the fact that tilted posture may mean tilt of the head, of the trunk, or both. Müller has described the evidence in detail (30). The Aubert phenomenon is reported as highly variable and not reliable. One of the writers (Gibson) has never been able to observe it in a clear-cut manner; the luminous line in the dark usually appears 'really' vertical whatever the position of the head.

that it can occur only when these are functioning. Moreover, as we shall see later, such observations are matched by experiments with precisely opposite implications—experiments in which the kinæsthetic and visual vertical prove to be very stable and quite precise in the absence of the v-factors.

Shift of the visual cues so as to conflict with gravity.—Better proof of the contribution of v-factors is to be found in observations where the visual scene is tilted out of alignment with the axis of gravity. Hemholtz reported that passengers on shipboard see the cabin as stationary while a suspended barometer (*i.e.*, a pendulum) seems to sway (**38**, p. 250). Innumerable landlubbers have observed the slow wheeling of the horizon beyond the fixed deck of the ship—and gone below in haste. The framework is undoubtedly anchored to the visual lines in this situation, but as Helmholtz himself went on to observe, the situation is only temporary. When the passenger gets his 'sea legs,' the ship seems to move and the barometer (or the horizon) remains fixed. The postural and the visual vertical for the veteran sailor are anchored to gravity.

A case where the ascendency of vision over gravity may seem even more striking is to be found in the 'haunted' or 'magic' swing, an amusement-park device. The 'swing' itself is usually wide enough to seat several persons side by side and is rigidly suspended from a heavy metal shaft passing horizontally through the center of a large, often elaborately furnished room. When the 'passengers' are seated and have been strapped in, they are told that they will be swung violently back and forth and that the swing will occasionally make a complete revolution. Actually the swing remains at rest, except perhaps for a little jostling; it is the room, with the furniture secured to the floor, that is put into motion. However, the illusion is quite compelling and persons often struggle violently to make the equilibritory adjustments which the visual stimulation seems to call for. The strapping of the 'passengers' to the seat is necessitated, not by the motion of the seat itself, but because of the danger that, in attempting to orient on the basis of the visual cues presented

by the moving room, they may throw themselves out of the seat. In an article written in 1895, Wood (**40**) makes the following significant comments regarding this illusion:

> The curious and interesting feature however, was, that even though the action was fully understood, as it was in my case, it was impossible to quench the sensations of 'goneness within' with each apparent rush of the swing. The minute the eyes were shut the sensations vanished instantly. Many persons were actually made sick by the illusion. I have met a number of gentlemen who said they could scarcely walk out of the building from dizziness and nausea. I myself experienced no sensations of dizziness, being accustomed to heights and to rapid motion; but the sensation before described was always present (and I visited the place several times), though I tried to suppress it and reason against it.

These examples of a shift in the v-factors are complicated by the fact that they involve motion. It is very doubtful that the 'magic swing' illusion would persist if the moving room were brought to rest in an oblique position. The occupants would surely recover an equilibrium based on gravity. Nevertheless the question arises, does the spatial framework attach itself to visual lines when these are objectively tilted but by an unchanging amount? Koffka believes that it does (**23**, p. 217). If one travels up a steep slope on a mountain railway and looks through the window of the railway car, the trees, telegraph poles and houses framed by the window often appear to be tilted rather than upright. The suggestion is that the car and the window frame dictate the framework and hence objective verticals and horizontals must necessarily look tilted.

Another illusory impression is that of the slope of a river flowing between canyon-walls composed of tilted rock-strata. A remarkable example is found in Ausable Chasm, New York, where the sightseer is taken downstream in a small boat. Here, the strata constituting the main lines of the visual field slope *against* the stream, and consequently the sightseer feels that he is 'shooting a rapids' at a fearsome degree of inclination.

It is easy to assume in these examples that the principal visual lines which are objectively tilted (window, strata) straighten up completely and thereby inevitably cause the lesser lines in the field which are objectively upright (trees, houses, stream) to appear tilted by precisely the same invariant amount. But this assumption has apparently never been given any experimental verification. Possibly all that these observations make certain is that under given conditions *upright visual objects look tilted*, and this is not the same thing as saying that they do so *because tilted objects look upright.* There is in fact experimental evidence to contradict the assumption. If a subject looks through a wide paper cone at a visual field filled with inclined lines, there will be induced a simultaneous contrast effect on a single objectively vertical line in this field; that is to say, the single line will look tilted in the opposite direction. The significant fact is, however, that the inclined lines continue to look inclined even after prolonged inspection (**17**, p. 557). If the tilted visual field does not right itself under experimental conditions, it may not do so under uncontrolled conditions, given sufficiently attentive observation, and in this event our examples had better be ascribed to a *contrast phenomenon* rather than to a shift of the framework into alignment with the field.[3]

Wertheimer's experiment.—The strongest claim for a completely labile visual framework shifting readily in accordance with the lines of visual organization comes from Wertheimer (**39**). He had a subject look through a tube into a mirror so placed that the image of the room behind appeared tilted by 45°. At first the room appeared thoroughly disoriented; a man seemed to be walking about on a tilted floor, an object fell in an oblique line, etc But 'after a few minutes' there was a change, he reports, and the room looked upright, with the floor horizontal and the path of falling objects vertical. Wertheimer's interpretation was that the

[3] The possibility that the simultaneous contrast phenomenon is itself to be regarded as a slight *partial* shift of all stimulus values on a continuous scale cannot be disregarded. But it seems equally probable that it is an *increase of difference* between two values on a scale. *Cf.* Koffka's discussion of color-constancy (**24**).

lines of the mirror-image served as 'anchoring forces' determining the subject's vertical and horizontal. His description is unequivocal, although many things one might wish to know about the phenomenon are not stated. A test was made of the subject's ability to set a white line on a circular black ground to the vertical position before and after the main experiment, but these results were not so clear-cut. After the tilted room had righted itself, the line could not be made vertical with the same certainty as before; instead it fluctuated and showed great instability.

Unquestionably, this phenomenon of Wertheimer's supports Koffka's hypothesis that "the main directions of the field constitute the framework" (**23**, p. 215). The mirror-room when first seen appears tilted because the subject is still in his normal framework, in which vertical retinal lines produce vertical phenomenal lines. "But this old normal framework has no support in the mirror world and it cannot maintain itself without such support. Instead, the new main lines of organization take over the role of creating the framework: the mirror world right itself" (**23**, p. 216). In this hypothesis, visual space is labile, relativistic, and completely anchored to, or determined by, processes of visual organization.

One of the writers (Gibson) has repeated Wertheimer's experiment in the effort to verify the phenomenon. A four by six foot mirror at one end of a large room was shifted out of the frontal plane of the subject by rotation around two axes. The top was tilted back and the left end shifted forward. The subject sat looking into this mirror through a cardboard cone fitted to his face which excluded the frame of the mirror but still permitted a 40° visual field. The mirror-image of the room was tilted approximately 35° counter-clockwise. Written descriptions were made of several sessions, lasting up to 15 minutes.

Usually within a minute after the beginning of the observation the tilted room began to look more *natural* than it had at first. It still looked tilted however, and this character remained unchanged throughout the session. With

effort, the subject could attempt to *project himself into* the mirror room and at such times it looked still more natural—less foreign, artificial, or 'wrong.' Such moments could not be maintained for long. The subjective vertical and horizontal were always capable of being called up and used to estimate the inclination of the room. Visually the subject felt himself in or at the end of this room, but the chair on which he sat and the floor which he felt were not a part of it, since they were upright while the room was tilted. The chair and the floor never became congruent with the visual field; the postural 'feel' of the body, no matter how much one might try to neglect it, was an omnipresent factor in the seeing of the room and remained incompatible with it.

At the moments of 'projection' referred to there was a strong involuntary tendency to bend the head in the direction of the room's inclination. If this impulse were yielded to, however, the tilt of the head into alignment with the room seemed to enter into the situation still to prevent the room's looking upright. The tilt of the room persisted, although now changed in character and somewhat decreased in amount. An interpretation of this fact would be that 'perceptual constancy' can preserve the orientation of a tilted environment as well as that of a normal environment. The constancy is incomplete in this situation as it is also in the Aubert Phenomenon.

In order to verify the main results with more subjects and also to try the effect of tilting more lifelike scenes including people and movement, a simple optical system built by the other writer (Mowrer) was employed. It consisted of a pair of totally reflecting right angle prisms mounted in short tubes, one in front of each eye, giving a circular visual field of 28°. The field was reversed from right to left but this fact did not seriously affect the experiment. By rotating both tubes through an appropriate angle, any amount of inclination of the field could be obtained. This device is a simpler version of one used by Brown (**5**). Three different scenes were observed at degrees of tilt varying from 10° to 45°. They were (1) a room filled with apparatus seen through a wide doorway,

(2) a hallway with people entering and leaving doors, and (3) a lawn with trees and part of a building. Five subjects were used although not in all combinations.

The reports completely substantiated the experiment with the mirror. After the first moments when the tilted field appeared unnatural, it began to look real and objects to look familiar. In this state whether the field appeared actually less tilted or only less strikingly tilted was hard to estimate. At any rate the tilt did not disappear; at best it was 'almost not noticed.' Movement of objects in the field seemed to facilitate this condition. No negative after-effect of tilt was reported for the visual field after the conclusion of the experiment, nor any serious instability of the field.[4]

These results are even further confirmed by those of Brown (**5**), who wore prisms similar to ours for one week, the field being inclined 75° from the vertical. Although he reports that the tilting of the world was less noticed as time went on, the new situation never became perfectly normal. Spatial behavior became very much easier but the phenomenal inclination persisted.

We are forced to conclude from all this evidence that a tilted visual field does not right itself under the conditions described and that possibly Wertheimer was reporting a relatively more cognitive phenomenon—the decrease in the strangeness and unreality of the mirror-scene. Such an interpretation is compatible not only with the evidence cited but also with the observation of Stratton and Ewert on the complete inversion of the visual field (**35, 10**).

In summary, the evidence for the anchorage of the perceived vertical to the v-factors consists of these facts: posture is often unstable when vision is eliminated; the perceived vertical, both visual and postural, is often disturbed or even destroyed when the main visual lines move with respect to gravity; and the perceived vertical is partially shifted when

[4] There is no contradiction involved between these results and Gibson's findings with respect to prolonged inspection of tilted lines. The negative after effects in the latter case seem to be localized shifts relative to the field as a whole, *i.e.* are confined to the stimulated region of the subjective visual field (**17**).

the main visual lines are tilted by a constant amount (possibly simultaneous contrast). A shift of the perceived vertical into complete congruence with such main lines has not been verified.

Evidence for the Dependance of the Vertical on Gravitational Determinants

Elimination of visual cues.—Although, as we have seen, there are observations tending to show that the postural and the visual vertical are weakened in the absence of vision, there are on the other hand experiments demonstrating that the postural and the visual vertical are both stable and precise with vision eliminated. The first of these is Garten's investigation (**14**). Garten built an elaborate tilting-chair on which the subjects sat blindfolded. The chair was first tilted and then set moving slowly toward the normal position; the subject had to stop it when he and the chair felt upright. The accuracy with which this could be done was surprising. After a little practise the average error amounted to less than one degree. The postural vertical, in short, is sensed with great precision. This is the fact in which we are primarily interested.[5]

The visual vertical, too, is sensed with great precision in the absence of any visual cues to anchor the framework. Neal (**31**, 291) found that for subjects in complete darkness, when the framework had been given a presumable opportunity to become unstable, an adjustable luminous line could be set to the vertical with an average variation of only .94 degrees. This figure was based on 720 readings from each of three subjects. She concludes that "localization of the vertical in the dark must depend almost entirely upon the absolute vertical, and the position of the body, and not to any appreci-

[5] Garten went on to isolate the various sensory factors on which this performance depended. He believed that the otolithic function did not play an important part since subjects with defective inner ears were capable of it. And since immersing the subjects' body in water did affect the performance, while anæsthetizing the skin areas in contact with the chair (*e.g.*, cooling the buttocks) did not, he concluded that tactual sensitivity was also eliminated and that the muscle sense is chiefly responsible for this kind of orientation.

able extent upon points of anchorage in the visual field." The similarity in acuity between Garten's and Neal's results is striking, suggesting once more a common basis for both functions.

Helmholtz (**38**, p. 256), Jastrow (**22**), and Gibson and Radner (**18**), among others, have found that in an illuminated field of view the high acuity for the vertical position of a line (a fraction of one degree) does not depend on the presence of other verticals or horizontals in the field but is a seemingly self-contained function. Furthermore Gibson showed that although the normal retinal-phenomenal correspondence for perception of the vertical may apparently be shifted under the influence of a process similar to sensory adaptation with negative after effect, the maximum change is slight and is confined to the stimulated area (**17**). The framework *as a whole* manifests great stability and resistance to such a change in correspondence.[6]

Shift of the direction of gravity so as to conflict with vision.— In a series of investigations going back more than a century, a number of observations have been reported concerning the subject's spatial orientation when a horizontal centrifugal force is added to the vertical gravitational force. The first published observations of this kind are apparently those reported by Johann Purkinje (**32**, p. 88) in 1820. This writer noted while riding on a carousel, or merry-go-round, that the entire revolving platform seemed somewhat inclined, the point where he was standing appearing lowest and the corresponding point on the opposite side appearing highest. This illusion, says Purkinje, "is so striking and vivid that one involuntarily leans decidedly to one side [toward the center]," in order to bring the long axis of the body into line with the perceived vertical. Purkinje appeared to regard this distortion of the perception of the true vertical as connected in some way with the effect of centrifugal force upon

[6] It may be noted that, in contrast with adaptation and negative after-effect, the shift involved in maintaining 'constancy' of the framework is instantaneous and completely compensatory. This shift from the normal retinal-phenomenal correspondence which occurs when the retinæ are tilted with the head is the more remarkable in view of the resistance to change when normal posture is maintained.

the blood and the semi-liquid contents of the cranium (*cf.* Cattell, **8**, 1895). More important to us, however, is the observation that the perceived vertical will alter when the direction of gravity is altered. Approximately fifty years after the publication of Purkinje's observations, Ernst Mach (**29**, pp. 286–287), became much interested in this problem; the circumstances under which his interest was aroused he described in the following words:

> I was rounding a sharp railway curve once when I suddenly saw all the trees, houses, and factory chimneys along the track swerve from the vertical and assume a strikingly inclined position. What had hitherto appeared to me perfectly natural, namely, the fact that we distinguish the vertical so perfectly and sharply from every other direction, now struck me as enigmatical. Why is it that the same direction can now appear vertical to me and now cannot? By what is the vertical distinguished for us?

These reflections inspired Mach to construct an apparatus in which he could have himself enclosed in a seated position in a large 'paper box' and uniformly rotated in the horizontal plane several feet removed from the axis of rotation. After the feeling of rotation occasioned by the initial acceleration of the apparatus had had sufficient time to disappear, it seemed to Mach that the box in which he was inclosed was at rest but considerably tilted, the floor of the box sloping downward in the direction away from the axis of rotation, *i.e.*, outward. However, with the stopping of the apparatus, the box seemed promptly to 'straighten up.' The main lines of Mach's visual field—the edges and corners of the box—were apparently ineffective in determining the vertical. On the basis of these observations, Mach concluded (**28**, p. 27) that whenever a horizontal mass acceleration, due to the centrifugal force, is added to the constant vertical mass acceleration, due to gravity, "one senses the direction of the resultant mass acceleration [which is determined by the law of the parallelogram of forces] and regards this as the vertical."

In his experiment Mach did, however, observe that when

a pendulum was suspended on the inside of the special 'box' which he employed, the direction of the apparent vertical during uniform rotation *usually coincided* with the position assumed by the pendulum; but this was not invariably the case. Thus, "it occasionally seemed to me as if the vertical was located between the direction of the pendulum and the axis of my own body" (**28**, p. 28). Mach apparently attached little significance to this discrepancy, however, and made no attempt to account for it.

There then followed a number of studies, notably those of Delage (**9**), Kreidl (**25**), Breuer (**3**), Breuer and Kreidl (**4**), Alexander and Barany (**1**), and Sachs and Meller (**33**), in which the problem raised by Purkinje and later by Mach as to which receptors mediate the sensations produced by gravity, or gravity and centrifugal force in combination, was hotly debated. During this period a new factor was introduced into the discussion, namely, the role of the *Raddrehung*, or counter-rolling, of the eyes which was discovered to be a reflex function of the position of the head with reference to the direction of the pull of gravity. Although this reaction was shown to be primarily dependent upon the vestibular receptors, its discovery introduced a new visual factor into the analysis of the perceived vertical. If, as a result of the existence of a horizontal centrifugal force added to the vertical pull of gravity, the resultant mass acceleration acting on the body occurs in an oblique direction, and if this rotates the eyes reflexly in their sockets so that their vertical meridians tend to come into alignment with the direction of this acceleration, then this distorted position of the eyes might be interpreted as responsible for the tipped appearance of the visual field. This explanation assumes that the perceived vertical should correspond with the vertical meridians of the retinæ—an assumption which was later proved to be false.

In their joint investigation Breuer and Kreidl (**4**) rotated human subjects in an enclosed compartment at the speed of one revolution in 5.5 seconds and at a distance of 2 meters from the axis of rotation, under which conditions the direction of the resultant mass acceleration acting upon the subjects

formed an angle of about 15° with the true vertical. Just prior to the onset of rotation, Breuer and Kreidl required each subject to fixate a vertical platinum wire, heated to the point of luminosity, for a period sufficiently long to insure a fairly enduring after-image. After the nystagmus which was occasioned by the onset of rotation had disappeared, each subject was required to adjust a pointer on the inside of the compartment in which he was seated so that it coincided with the after-image of the platinum wire. The three subjects used by Breuer and Kreidl showed, on the average, a deviation of the after-images of about 8°. In their experiment the position of the after-images always seemed to coincide perfectly with what appeared to the subjects to be the true vertical. These writers concluded that "it is certain, therefore, that the apparent tilting of the visual field is caused by a real, unconscious counter-rolling (Raddrehung) of the eyes" (**4**, p. 499). This conclusion is contradicted by subsequent investigations, as we shall see. It is to be noted that the tendency of the perceived vertical to coincide with the changed direction of gravity is only partially effective in Breuer and Kreidl's experiment.

The efforts of the various countries participating in the World War to bring their respective aviation forces to the highest possible point of efficiency resulted in a number of studies of the influence of the different types of aeroplane maneuvers upon the perception of the vertical and of the different sensory mechanisms involved in the efficient piloting of these machines. Observations reported by Head (**20**), van Wulfften Palthe (**37**), Leiri (**26**), Bauer (**2**), Tschermak and Schubert (**36**), Gemelli (**16**), Burtt (**6**), and others at this time brought out the fact that during properly executed 'turns,' 'spirals,' and even 'loops,' a passenger—if vision of the horizon is excluded—is unable to sense any departure from a normal position with reference to the direction of gravity. When vision is allowed the passenger may either feel that the horizon tilts while the aeroplane retains a constant orientation with respect to the vertical, or that the horizon retains its usual horizontal position, with the aeroplane

and its cargo undergoing positional changes. Even experienced pilots appear to vary greatly in their reactions in this respect.

Following the World War the problem of the perception of the vertical was again taken back into the laboratory, notably by Garten (**14**), Fisher (**11**, **12**, **13**), Shoda (**34**), and Tschermak and Schubert (**36**). These writers were especially concerned with a re-examination of the theory of Breüer and Kreidl concerning the role of the vestibulo-ocular reflexes in determining the perception of the vertical. In general their conclusions may be represented by the statement of Tschermak and Schubert "that no strict relationship exists between the objective change in eye position and the localization of the apparent vertical" (**36**, p. 240) and by the contention of Garten that the receptors of the non-acustic labyrinth in themselves are probably relatively unimportant for the perception of the vertical.[7] There is no question, however, of the validity of the original phenomenon discovered by Purkinje and Mach.

Orientation in a 'motordrome.'—A striking opportunity to observe the influence of centrifugal force upon the perception of the vertical (and one which, so far as the writers are aware, has not previously been taken advantage of) is that afforded by the so-called 'motordrome,' where vehicles travel around the inside wall of a large vertical cylinder.

In order to obtain some notion concerning the sensory impressions of persons who perform in these motordromes, one of the writers (Mowrer) has questioned several such

[7] This is not to say that the non-acustic labyrinth is entirely unrelated to the maintenance of posture and the perception of verticality; it is simply *one* of the several sensory mechanisms which normally contribute to this complex function. The fact that *abnormal* stimulation of the labyrinth may rather profoundly disturb equilibration does not, as some writers have supposed, prove that the labyrinth is the sole mediator of the perceptual and postural vertical. It is well known, for example, that if a person is rotated for a time with the head, let us say, tilted sharply forward and is then told upon stopping to raise the head, a violent 'falling reaction' to one side and accompanying disturbances of the perceived vertical will ensue. But it is quite as fallacious to conclude on the basis of this or similar facts that the labyrinthine receptors are the *exclusive* mediators of posture-determining stimuli as it would be to conclude from some of the other observations cited above that the eyes are alone important in this connection.

persons and on two occasions has accompanied one of them. In a 'drome' with a diameter of 40 feet, a motorcycle traveling around the vertical sidewall at a speed of 35 m.p.h. is acted upon by a horizontal centrifugal force equal to approximately 3.9 times gravity. Naturally one's 'weight' under such conditions is relatively tremendous; the task of lifting a hand (which feels as if it might be made of lead) involves exorbitant effort. At still greater speeds the blood is centrifuged from the head, with ensuing cerebral enemia and loss of consciousness, the occurrence of which is presaged by what the riders refer to as 'going blind.' Following a ride in a motordrome there is also an interesting form of post-rotational nystagmus which, however, cannot be pertinently discussed here.

Without exception performers in a motordrome report the feeling that they are riding in a virtually upright position, with the motordrome lying on its side and rolling along barrel-fashion. The actually vertical wall of the motordrome 'beneath' them is perceived as perfectly horizontal. Therefore, since the vector resultant formed by centrifugal force and gravity is not quite perpendicular to the wall of the motordrome (deviating about 14° at a speed of 35 m.p.h. in a 'drome' of the size indicated), a motorcycle rider will feel that he is tilted slightly away from his perceived vertical, which is perpendicular to the wall. Experienced riders learn to accept this unnatural position, but for the neophyte it is extremely disconcerting, causing him continually to be trying to 'straighten up' (*i.e.*, sit in an actually horizontal position) which of course interferes with the balancing of the motorcycle.

On the first occasion when the writer accompanied a professional rider in a motordrome, the only describable experience was one of complete spatial disorientation. Movement was clearly perceptible but it was impossible to give it any definite directional reference. It was as if a complexly integrated perceptual mechanism had completely broken down; and this, we conjecture, is precisely what had happened. Visual and postural factors were thrown into such violent conflict that no stable perceptual organization could emerge.

However, on the occasion of the second ride (which occurred only a few minutes after the first), this confusion disappeared, and the spatial illusion reported by veteran riders was vividly experienced.

Here it is significant to note that although this illusion, that the motordrome is lying on its side and revolving in this position, clearly reveals the predominance of what we have called gravitational factors over visual factors, nevertheless visual factors exert some influence on the perceived vertical. If the g-factors were the sole determinants in this situation the rider's apparent vertical would deviate just 76° from the real vertical, coinciding with the vector resultant. As it is, the rider's vertical deviates 90° from the real vertical, presumably under the influence of a visual tendency for the sidewall to determine the rider's horizontal. In other words we are accustomed to ride in a visually upright position on a horizontal surface, and within limits this visual factor exerts its own influence. A new perceptual integration is achieved, in which gravitational factors are the primary determinants but which is also influenced to some degree by visual cues.[8]

Discussion

The evidence which has been summarized would seem to support the hypothesis that both the framework of visual space and postural orientation are jointly determined by both visual and gravitational factors, but that the latter are more decisive. Not only do the facts point to this conclusion but also there is an argument for it which we have not yet brought to bear. If one goes back and examines the two lists of stimuli outlined at the beginning of the paper, the g-factors and the v-factors, it becomes evident that the visual stimuli can have specificity for either postural or phenomenal response-processes only if a *fixed posture of the eyes is established.* Visual lines are not in their own right stimuli for orientation

[8] It must be kept in mind that the intensity of the total mass acceleration acting upon the body in a motordrome is several times greater than normal gravity. Because of this augmentation of intensity, it is possible that spatial perception is here influenced somewhat more by what we have called gravitational factors than would be the case if the direction of gravity could be changed without also being intensified.

If the eyes rolled at random within the head, if the organism could not be oriented to gravity, a vertical line of stimulation on the retina would be neurologically meaningless. Logically, therefore, visual orientation presupposes postural orientation. Once orientation to gravity is granted, visual cues can make their contribution both to posture and to the uprightness of seen space; they become specific. Only at this stage of reasoning does it become possible to postulate a mechanism for the automatic correction of visual cues in conditions of sensed abnormal posture. The visual framework can now be subject to 'constancy,' or in other words, tilted retinal lines can yield upright phenomenal lines when the head is correspondingly tilted. Once there is specificity of stimuli there can be orderly variations in this specificity, but not before. The priority of the g-factors to the v-factors is thus a logical matter. That they are also genetically prior is indicated by the fact that geotropisms precede pattern-vision in the evolution of sensory functions.

Vision 'fills in' the framework, as it were, but its skeleton tends to be fixed by gravity. Undoubtedly visual pattern-stimulation has some secondary part in helping to 'anchor' visual space but its basic anchorage is to the postural vertical. Spatial perception, in other words, is mediated by postural mechanisms. And this is necessarily so, since essentially what we are saying is that *there can be no phenomenal space without posture.*

A fundamental difficulty with the older theories of space-perception was that they strove first to explain how space could be sensed, experienced, or seen—what the 'cues' were for perception—and then on the basis of these experiences the appropriate spatial behavior was supposed to be explicable. But if perceptual space is a motor phenomenon through and through, if the spatial experiences and sensed relations are reflections of discriminative response just as truly as the other way around, then this line of attack was a mistaken one. It may be argued that sensed cues, judgments, or experiences should not be made the basis for spatial behavior for the reason that they are themselves already spatial behavior.

A man who crosses a stream by jumping from rock to rock without wetting his feet illustrates a psychological performance whose perceptual and behavioral elements are inseparable. It should not be accounted for in terms merely of his correct sense of the vertical or of his excellent equilibritory reactions for, properly considered, these are both abstract features of a unitary function in which the behavior is not merely spatial but the space is behavioral.

Conclusions

I. The vertical and horizontal axes of perceived space have no empirical meaning except with reference ultimately to the direction of terrestrial gravity.

II. Certain visual lines (the main lines of the visual field) normally bear a fixed relation to the direction of gravity.

III. Phenomenal uprightness is a complex feature of perception, dependent upon the integration of variable visual and postural (gravitational) determinants.

IV. When the head is upright with respect to gravity (or tilted so little that the reflex counter-rolling of the eyes adequately compensates for the tilt) the vertical and horizontal lines of stimulus-objects fall upon the vertical and horizontal meridians of the retinæ; when the head is tilted markedly this congruence is destroyed, yet there is no distortion of the perceived vertical (the constancy phenomenon). Presumably this results from the concomitant alteration of postural stimuli.

V. When the main lines of the visual field are eliminated, the postural vertical may sometimes be disturbed; nevertheless under experimental conditions both the postural and the visual vertical are sensed with a high degree of acuity in the absence of vision.

VI. When the main lines of the visual field are artificially shifted with respect to gravity by a fixed amount, a corresponding shift of the perceived vertical may occur; but the shift is relatively slight and the visual field does not, as has been claimed, 'right itself' under these conditions.

VII. When the main lines of the visual field are shifted

with respect to gravity more or less continuously (remain in motion), instability of the perceived vertical is likely to occur and to be conspicuous.

VIII. When the direction of the pull of gravity is considerably altered by the addition of a centrifugal force and the main visual lines remain unaltered, a deviation of the perceived vertical always occurs by an amount which sometimes approaches the degree of alteration.

IX. When the direction of gravity and the main visual lines are both *concomitantly* altered, in the same direction and by the same amount (as in a 'banking' train or cabin airplane), the perceived vertical deviates correspondingly by the same amount, as would be expected.

X. Gravitational (postural) factors are both logically and psychologically prior to visual factors in the determination of the perceived vertical. The framework of visual space is derived from and dependent upon the necessity of maintaining postural equilibrium against the pull of gravity.

REFERENCES

1. Alexander, G. und Bárány, R., Psychophysiologische Untersuchungen uber die Bedeutung des Statolithenapparates für die Orientierung im Raum an Normalen und Taubstummen, nebst Beiträgen zur Orientierung mittels taktiler und optischer Empfindungen, *Zsch. f. Psychol. u. Physiol. d. Sinnes.*, 1904, **37**, 321–362 and 414–457.
2. Bauer, L. H., Dangers of high speed and acrobatic flying, *Aviation*, 1924, **16**, 174–176.
3. Breuer, J., Ueber die Function der Otolithen-Apparate, *Pflüg. Arch. f. d. ges. Physiol.*, 1891, **48**, 195–306.
4. —— und Kreidl, A., Ueber die scheinbare Drehung des Gesichtsfeldes während der Einwirkung einer Centrifugalkraft, *Pflüg. Arch. f. d. ges. Physiol.*, 1898, **70**, 494–507.
5. Brown, G. G., Perception of depth with disoriented vision, *Brit. J. Psychol.*, 1928, **19**, 117–146.
6. Burtt, H. E., The perception of slight changes of equilibrium, with especial reference to problems of aviation, *J. Appl. Psychol.*, 1918, **2**, 101–115.
7. Carr, H. A., An introduction to space perception, New York: Longmans, Green, 1935, Chap. 9.
8. Cattell, J. McK., The sense of equilibrium, *Science*, 1895, **2** (new series), 99–100.
9. Delage, Y., Etudes expérimentales sur les illusions statiques et dynamiques de direction pour servir a déterminer les fonctions des canaux semicirculaires de l'oreille interne, *Arch. d. zool. expér. et gén.*, 1886, **4**, 535–624.
10. Ewert, P. H., A study of the effect of inverted retinal stimulation upon spatially coördinated behavior, *Genet. Psychol. Monog.*, 1930, **7**, 177–363.

11. Fischer, M. H., Messende Untersuchungen über die Gegenrollung der Augen und die Lokalisation der scheinbaren Vertikalen bei seitlicher Neigung (des Kopfes, des Stammes and des Gesamtkörpers), *von Graefes Arch. f. Ophthalmol.*, 1927, **118**, 633–680.

12. ——, Die Regulationsfunktionen des menschlichen Labyrinthes und Zusammenhänge mit verwandten Funktionen, *Ergeb. der Physiol.*, 1928, **27**, 209–379.

13. ——, Messende Untersuchungen über die Gegenrollung der Augen und die Lokalisation der scheinbaren Vertikalen bei seitlicher Neigung des Gesamtkörpers bis zu 360°. II Undersuchungen an Normalen, *von Graefes Arch. f. Ophthalmol.*, 1930, **123**, 476–508.

14. Garten, S., Über die Grundlagen unserer Orientierung im Raume, *Abhandl. d. Math.-Phys. Klasse de Sächs. Akad.*, 1920, **36**, 431–510.

15. Gelb, A., Die psychologische Bedeutung pathologischer Störungen der Raumwahrnehmungen, Jena: *Ber. ub. d. IX Kongr. f. Exp. Psychol.*, 1926, 28–32.

16. Gemelli, A., L'orientazione lontana vel volo in æroplano, *Riv. di psicol.*, 1933, **29**, 297–325.

17. Gibson, J. J., Adaptation, after-effect, and contrast in the perception of tilted lines. II. Simultaneous contrast and the areal restriction of the after-effect, *J. Exper. Psychol.*, 1937, **20**, 553–569.

18. —— and Radner, M., Adaptation, after-effect, and contrast in the perception of tilted lines. I. Quantitative studies, *J. Exper. Psychol.*, 1937, **20**, 453–467.

19. —— and Robinson, D., Orientation in visual perception; the recognition of familiar plane forms in differing orientations, *Psychol. Monog.*, 1935, No. 210, 39–47.

20. Head, H., The sense of stability and balance in the air, *Reports of the Medical Research Committee of Great Britain*, 1918.

21. Jackson, E., Vision for equilibrium and orientation, *Amer. J. Ophthal.*, 1933, **16**, 415 ff.

22. Jastrow, J., On the judgment of angles and positions of lines, *Amer. J. Psychol.*, 1893, **5**, 220 ff.

23. Koffka, K., Gestalt psychology, New York: Harcourt Brace, 1935.

24. ——, Some remarks on the theory of color constancy, *Psychol. Forsch.*, 1932, **16**, 329–353.

25. Kreidl, A., Beiträge zur Physiologie des Ohrlabyrinths auf Grund von Versuchen an Taubstummen, *Pflüg. Arch. f. d. ges. Physiol.*, 1892, **51**, 117–150.

26. Leiri, F., Über die Bedeutung des Vestibularapparats bei der aviation, *Zsch. f. Hals-, Nasen-, und Ohrenheilkunde*, 1927, **17**, 381–391.

27. Mach, E., Physikalische Versuche über den Gleichgewichtssinn des Menschen, *Sitzungsberichte der kaiserl. Akad. der Wissens.*, 1873, **68**, 124–140.

28. ——, Grundlinien der Lehre von den Bewegungsempfindungen, Leipzig: Wilhelm Engelmann, 1875, pp. 128.

29. ——, Popular Scientific Lectures (Trans. by T. J. McCormack), Chicago: Open Court, 1898, 282–308.

30. Müller, G. E., Über die Aubertsche Phänomen, *Zsch. f. Sinnesphysiol.*, 1916, **49**, 109–244.

31. Neal, E., Visual localization of the vertical, *Amer. J. Psychol.*, 1926, **37**, 287–291.

32. Purkinje, J., Beiträge zur näheren Kenntniss des Schwindels aus herautognostischen Daten, Wien: *Medicinische Jahrbücher des kaiserl. königl. österreichischen Staates*, 1820, **4** (ii), 79–125.

33. Sachs, M. und Meller, J., Untersuchen über die optische und haptische Lokalisation bei Neigungen um eine sagittale Achse, *Zsch. f. Psychol. und Physiol. der Sinnes.*, 1903, **31**, 89–109.
34. Shoda, M., Über die scheinbare Vertikale bei Tertiarstellung des Auges, *Pflüg. Arch. f. d. ges. Physiol.*, 1926, **215**, 588–607.
35. Stratton, G. M., Vision without inversion of the retinal image, Psychol. Rev., 1896, **3**, 611–617; 1897, **4**, 341–360, 463–481.
36. Tschermak, A. und Schubert, G., Über Vertikalorientierung im Rotatorium und im Flugzeuge, *Pflüg. Arch. f. d. ges. Physiol.*, 1931, **228**, 234–257.
37. van Wulfften Palthe, P. M., Function of the deeper sensibility and of the vestibular organs in flying, *Acta Otolaryngologica*, 1922, **4**, 415–449.
38. Von Helmholtz, H., Physiological Optics (Trans. by J. P. C. Southall), vol. 3.
39. Wertheimer, M., Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 1912, **61**, 257 ff.
40. Wood, H. C., The 'haunted swing' illusion, Psychol. Rev., 1895, **2**, 277–278.

[MS. received November 15, 1937]

THE POSTURAL SUBSTRATE[1]

BY G. L. FREEMAN

Northwestern University

I

The postural substrate is one of the most neglected, most misunderstood and most important aspects of behavior. Those who account for functional performance in terms of superficial S–R 'bonds' tend to ignore this organic component altogether. Those who recognize that stimulation effects are superposed upon activity already present tend to sweep these tonic bodily trends into some system of mental energetics where their interpretation becomes confused. It is significant that until recently the problems of 'energy mobilization,' 'reactivity level' and 'excitation background' have been considered outside the province of objective psychology and that any information about the dynamics of behavior has had to be gained from the writings of Freud and other offenders of orthodox scientific reasoning. Available evidence now indicates that the tissue activities which compose the postural substrate provide an arena for experimental attack on reaction dynamics. The purpose of this paper is to define the arena and to suggest what appear to be promising modes of approach.

Before progressing to a detailed description of the postural substrate, it is well to point out its general significance. The fact that response to external stimulation is not strictly phasic in character but built on tonic patterns of activity gives these more enduring physiological states an unique functional significance. Not only do they insure a consistent adjustment from moment to moment; they are also an essential facilitating and sustaining condition for the occurrence of overt reaction. For just as a ball dropped upon a rubber band will

[1] *Studies in Muscular Tension*, series 2, no. 4.

not rebound unless the band is in a state of stretch or tension, so a 'stimulus' will not produce 'response' unless the organism's energies are appropriately mobilized. One might carry the analogy even further and say that just as the height of the ball's rebound is a function of the stretch of the rubber band so is the quantity and quality of response associated with the level of organic reactivity which is met by the incoming stimulus.[2]

While there is no certain agreement, the balance of evidence favors the view that the postural segment of reaction is different in type from the overt phasic response. In the first place there are the physiological differentiations in terms of latency (unless the stimulus is anticipated, the tonic pattern is slower to develop), duration (the tonic response outlasts the phasic response) and spread (the tonic reaction is more diffuse and extensive than the phasic reaction). In the second place there are the neurological differentiations in terms of receptor (phasic responses are occasioned by stimulation of exteroceptors whereas the regulative action of the tonic processes occurs through interoceptive channels), central control mechanisms (the great-pyramidal system dominates phasic reaction and the extrapyramidal system dominates tonic reaction) and, possibly, effector (some writers trace the differentiation through to the muscle fiber[3]). In the third place there is little reason to believe that either class of reaction exists apart from the other; that is to say, any cross-section of behavior would in all probability reveal a phasic reaction sustained by tonic reactions, and not a tonic or a phasic reaction in isolation as would be the case if these terms applied to two extremes of a single scale.

[2] The writer's use of the term 'reactivity level' is practically synonymous with Darrow's 'excitation background' (*J. Gen. Psychol.*, 1937, 16, 285–309), and refers to the bodily changes preceding a given stimulus which are due to relatively persistent internal conditions.

[3] J. R. Hunt (*Arch. Neurol. & Psychiat.*, 1923, 10, 37–82) has maintained that the muscle fiber itself gives evidence of duality of structure and function, with the phasic movement dependent on its fibrillary structure and postural (tonic) fixation on its sarcoplasm. The author has elsewhere (*Introduction to Physiological Psychology*, 1934, 446–451) reviewed the evidence on tonic and phasic reaction systems, showing that the motor nerve and its associated muscle fibers are probably influenced by nerve terminals from both pyramidal and extrapyramidal systems.

Following the argument outlined above *the postural substrate may be defined as the general organic background (neuro-glandular-muscular) which operates to sustain and energize overt phasic responses.* In it are a vast array of processes whose significance for the initiation and direction of behavior is only vaguely appreciated. Most obvious and well known as to their dynamic influence on performance are the processes of *skeletal muscular tension.* Less thoroughly understood but of recognized importance are the *visceral tensions and postural changes in smooth muscle tissue.* Most obscure of all effects and probably most vital to a thorough understanding of the postural substrate are the *secretions of endocrine glands.* Although the details of their interrelations are far from clear, it has become increasingly apparent that the energies utilized in response adjustment are either derived from or mobilized by the tonic (as opposed to the phasic) activity of muscle and gland.

Some years ago Kempf [4] wrote an elaborate monograph to show how processes identical to those here ascribed to the postural substrate influence response adjustment. Beginning with the dichotomy of autonomic (tonic) and projicient (phasic) systems, he advanced the theory that "whenever the autonomic sensory-motor apparatus is disturbed or forced into a state of unrest, either through the necessities of growth, metabolism or endogenous or exogenous stimuli, it compels the projicient sensori-motor apparatus to so adjust the receptors in the environment as to acquire stimuli which have the capacity to produce adequate postural readjustments in the autonomic apparatus." In this statement lies not only a genesis of what is now generally known as the 'tissue need' theory of motivation but also a suggestion regarding the more general problem of energy mobilization. Kempf's autonomic 'unrests' do more than initiate the phasic activity calculated to restore organic equilibrium. They sustain and regulate the efficiency of these overt adjustive acts since "the constant tendency of the autonomic (tonic) apparatus is to

[4] E. J. Kempf, The autonomic function and the personality, *Nerv. & Ment. Dis.*, Monograph Series, 1921, No. 28, p. 1–2.

maintain the projicient (phasic) apparatus so as to acquire a maximum of affective gratification with a minimum expenditure of energy." We are told that "the healthy individual has an elastic though limited quotient of energy" and that in making adjustments he will have expended more or less of this quotient according to the magnitude of his demands (affective cravings) or the environmental obstruction.

Because of his concept of energy quanta, Kempf has sometimes been accused of returning to the 'metaphysics of Freud's libido concept' from which he sought escape. But the implications of quantum mechanics have now sufficiently permeated psycho-physiological thought so that the mere mention of the term 'energy mobilization' does not necessarily imply that the author is a vitalist. Energy as we meet it in the organism is produced by the processes of metabolism. Tissues use and give off more or less energy according to the rate at which they are metabolizing. A high level of reactivity in a tissue means a high rate of metabolism. One tissue derives energy for its own operations by drawing directly or indirectly upon the products of metabolism of other tissues. The blood and the nerves work together in carrying out these effects. Besides containing the materials essential for metabolic activity of any tissue (oxygen and glycogen), the blood transports the products of metabolism from certain tissues (glands, etc.) to other tissues (muscles and nerves) where the chemical stimulus acts directly to increase their metabolic activity or reaction level. The nerves convey the indirect effect of metabolic activity from one tissue to another. Thus increased reactivity (metabolism) in a muscle increases the afferent excitation from this muscle, and this in turn stimulates some other muscle or gland, or nerve tissue to increased reactivity (metabolism). From this analysis it should follow that tissues mainly engaged in tonic activity may increase not only the metabolic activity of one another but also of tissues primarily engaged in phasic activity and *vice versa*. Furthermore, the quantitative aspects of the organic energy flux are not to be found

in the basic metabolic products of food and oxygen but in the ability of the tissues to make use of them.

A tissue may operate only within a limited range of reactivities; if the rate of metabolic exchange falls below or exceeds such limits it dies. Thus the quanta of energy which *could* be mobilized in support of a given adjustment would be defined by the upper metabolic limits or reactivity levels of all the tissues engaged in that activity. The question both as to the exact limits of this process and the individual differences in the absolute quanta of energy mobilization is only rhetorically significant. The only sure test that these absolute upper limits had been reached would be the breakdown of certain tissues; and since the organism acts as an integrated unit with each tissue vital to the well-being of the whole, this would probably mean death. Even more serious is the absence of a suitable physiological method for assaying the absolute level at which such breakdown occurred. The metabolic rate of the intact organism as measured by any present means provides a rough and indifferent composite of the reactivities of all the tissues in the body. So while the reactivity of the higher neural centers might be increased almost to the breaking point (as in apprehension), reactivity in other tissues might decrease as rapidly and no certain registration be made.

It is more fruitful to inquire concerning relative levels of energy mobilization than to talk in terms of theoretically absolute limits. If a person is more reactive in one situation than in another, or if under similar external change his reactivity is greater than that of another, we may conclude that he is operating on a higher 'energy level' and that a greater 'excitation background' is met by the incoming stimulus.

II

What means may be utilized in studying energy mobilization and dynamogenetic activities of the postural substrate? We have already seen that available methods of recording

general metabolism are too gross and slow-acting to reveal much. Blood pressure, however, has long been a standard indicator of the tone of an important group of smooth muscles, and the reactivity or tension of skeletal muscle tissue is open to direct measurement, either by mechanical or electrical methods. The weakest point of such measures of skeletal activity is also their strongest, for, while the tension of one group of skeletal muscles may not be symptomatic of the state of the entire musculature, the fact that the record is highly specific makes it useful in localizing 'conditioned' tension patterns. Neuro-muscular reactivity may also be recorded directly from the peripheral nerves of the intact organism, though there is some doubt as to whether this is a better index of central nervous activity than the electromyogram.

The palmar skin resistance is an indicator which now shows great promise of expressing general changes in the activity of both skeletal muscle fibers and tissues more directly under the dominance of the autonomic nervous system. The fact that palmar sweating apparently not only summates sympathetic and para-sympathetic effects but also reflects excitation from centers of tonic muscular control give to the records of secretory activity an unique functional significance; [5] and with proper attention to controls, 'basal' readings of palmar skin resistances apparently provide a more accurate and delicate measure of the general reactivity level than do any present methods of recording metabolism.[6]

If one is interested in energy mobilization induced through different structuro-functional control systems, he will need to use a combination of measures, since tissue activities comprising the postural substrate express changes initiated in various neural centers. At least three distinct but interdependent control systems are generally recognized: (1) the direct central nervous control of the skeletal muscles; (2) the parasympathetic control of smooth muscles and glands; and

[5] C. W. Darrow, Neural mechanisms controlling the palmar galvanic skin reflex and sweating, *Arch. Neurol. & Psychiat.*, 1937, 37, 641–663.

[6] G. L. Freeman and L. Griffin, The measurement of general reactivity under basal conditions (in press).

(3) the sympathetic control of smooth muscles and glands.[7] By obtaining standard deviations of simultaneous recordings of blood pressure, palmar secretory activity and muscle action potentials it should be possible to express the relative activity of the three systems by index numbers derived from the following equations:

Since *blood pressure* expresses sympathetic activity minus parasympathetic activity and *palmar skin resistance* summates parasympathetic and sympathetic activity; then, as noted by Darling and Darrow,[8]

$$\text{Parasympathetic activity} = \frac{BP - G}{2} \quad \text{and} \quad \text{Sympathetic activity} = \frac{G + BP}{2}.$$

And since *potential changes* in skeletal muscle summate activity induced through the central nervous control with that induced indirectly as an autonomic overflow phenomenon, it should follow that

$$\text{CNS activity} = PC - \left(\frac{BP - G}{2} - \frac{G + BP}{2}\right).$$

The electroencephalogram provides another index of central nervous activity. The notion that its spontaneous rhythms reflect a cortical excitation-background set up by subcortical 'pacemakers' and tissue activities forming the postural substrate has strong support. High correlations between the frequency of the rhythm and the subject's general metabolic level and body temperature have been reported [9] and there is good qualitative evidence that conditions of intense alertness and lethargy stand at two extremes

[7] While the sympathetic control center has been definitely localized in the hypothalamus, the parasympathetic centers are not as clearly defined. Ranson localizes these anterior to the hypothalamus but admits (*Trans. Col. Phys. Phil.*, 1934, 11, No. 3.) that peripheral effects resulting from stimulation in this region may be due to excitation of descending fiber tracts from frontal cortex, etc. Another unsolved problem is the differentiation of pyramidal and extra-pyramidal effects in electromyograms.

[8] R. Darling and C. W. Darrow, Determining activity of the autonomic nervous system from measurements of autonomic changes, *J. of Psychol.*, 1938, 5, 85–89.

[9] D. B. Lindsley and B. R. Rubenstein, Relationship between brain potentials and some other physiological variables, *Proc. Soc. Exp. Biol. Med.*, 1937, 35, 558; H. Hoagland, Temperature characteristics of the 'Berger rhythm' in man, *Science*, 1936, 83, 84–85.

in blocking out brain rhythms. Beginning with the condition of a background of extreme excitation with no rhythmic action potentials, the series presumably goes from states of moderate excitation with the high-frequency-low-amplitude potentials, through states of moderate relaxation with slower frequencies and beginnings of irregularity to very deep sleep with very slow random changes in electrical potential. According to Jasper,[10] if the cortical excitatory state is raised too high by internal pacemakers or by external stimulation, excessive polarization changes in the tissues result in cathode blocking and the spontaneous rhythm is 'knocked down' or fails to appear; on the other hand, if the cortical excitation state falls to a certain low level, inhibitory anode blocking will occur in the tissue and the rhythmic changes will fail to appear. With limited external stimulation the recorded potential changes are largely determined by the humoral processes affecting the cortical cells and by neural impulses arriving from lower centers. It would be very difficult in a single experiment to separate these excitatory effects. However, a comparison of the rhythms in basal metabolic conditions with changes accompanying gradual increments in muscular tension should be helpful in factoring the equation.

Very little can yet be said for the value of biochemical measures in estimating reactivity levels and the degree to which organic energies are mobilized in a given situation. Ambiguous results have been reported on the relation of pH of the blood to reactivity, and tests for blood sugar and carbohydrate tolerance have not provided more reliable answers.[11] Urine and salivary analyses are even more gross. Undoubtedly, at some future time, when it is possible to make accurate hormone analysis of the blood, the biochemical attack will contribute much to an understanding of the intricacies of energy mobilization. At present it does not seem as immediately fruitful as methods suggested above.

[10] H. H. Jasper, Cortical excitatory state and variability in human brain rhythm, *Science*, 1936, **83**, 84–85.

[11] G. J. Rich, *J. Abn. & Soc. Psychol.*, 1928, **23**, 150–175; *Arch. Neurol. & Psychiat.*, 1928, **20**, 589–594.

III

The problems of reaction dynamics, in which the postural substrate presumptively plays a major role, may be divided roughly into three classes: (1) those concerned with the limits of optimal reactivity in different types of performance; (2) those relating variations in physiological reactivity to the quality and quantity of performance in the same individual; and (3) those dealing with individual differences in reactivity and performance. Of these the last interest has the greatest general significance, for therein probably lies a key to the physiological mechanisms involved in personality disorders and the neuroses. It is just here, however, that the analysis will be most involved.

Since the postural substrate serves as the background and 'carrier' of phasic response adjustments, its level of activity reflects both the intensity of environmental demand and the extent of organic resource. If, therefore, such tissue activities are higher in one individual than another, one may conclude either that he is facing greater demands or that he has less capacity to meet the same demands and must work his machine harder to turn out equivalent goods.

The fact that the activities of the postural substrate are related to both supporting and ambivalent tendencies makes it difficult to analyse a given record of bodily change. For guidance here we have resort to certain principles which presumptively operate in reaction dynamics. The first is that the effect of a displacing stimulus is to raise the reactivity level and that in the absence of such stimulation, reactivity tends to return to a 'basal' equilibrium maintained by more persistent tonic bodily conditions. The second assumes that the reaction to a displacing stimulus is relative to the basal equilibrium which is upset since the postural energies which are occupied in maintaining a given level of equilibrium are not available for meeting the disruptive stimulus. The third principle is that the basal level itself operates effectively only over a limited range and if it falls below or exceeds these limits response adjustment is impaired.

We may hypothecate that two factors determine in large measure the relation of an individual's reactivity level to performance. One is his potentialities as a source of energy mobilization, 'libido,' or drive, and if it were the only determining factor, it would probably operate so that the greater the aroused postural energies the greater the performance. There is, however, another factor in the equation and that is how well the system primarily concerned in phasic response adjustment can utilize and channelize the energetics of its activity. If we call this factor 'inhibitory control' and relate to it the integrative action of the higher cerebral centers at various excitation levels, we may then assume that their combined action is at its best in the middle range of reactivities. Since performance scores are largely affected by the cortical 'control' factor, while measures of postural activity are most directly reflections of the 'drive' factor, their plotted relation should give an inflected curve indicating that increments in postural 'input' support increasingly effective performance 'output' up to a certain point, after which more excessive reactivity is accompanied by performance of decreased effectiveness. Performance levels maintained at the beginning of the curve would be akin to sleep whereas those at the end of the curve would represent collapse.

The fruitfulness of this general argument is already appearing in experimentation. After an individual is carefully 'relaxed down' to a basal level, his postural tensions may be increased progressively with first facilitative and then inhibitory effects upon performance.[12] A similar relation appears when the individual is faced with a situation which makes gradually increasing demands.[13] The inflection point from facilitatory to inhibitory effects differs with the indi-

[12] While 'basal' reactivity is not actually recorded, it is assumed that the asymptotic curve of rest in its peripheral indicator (palmar skin resistance, etc.) measures the relatively persisting internal conditions which affect such reactivity or excitation background. *Cf.* G. L. Freeman, The optimal loading of muscular tension for various performances, *Amer. J. Psychol.*, 1938, (in press).

[13] G. L. Freeman, The facilitative and inhibitory effects of muscular tension upon performance, *Amer. J. Psychol.*, 1933, **45**, 17–52; G. L. Freeman, and C. W. Darrow, Insensible perspiration and the galvanic skin reflex, *Amer. J. Psychol.*, 1935, **111**, 55–63.

vidual and with the character of the task. There is indication that great significance may attach to the level of postural activity at which an individual does his 'peak' work and to the magnitude of reactivity increment occasioned by stimulation.[14] That is to say, in meeting a standard task or displacing stimulus, some persons would mobilize relatively little postural input and use it to better advantage than those who mobilized excessively. By experimental loading of the nervous system (as in the inhibition of micturition and the endurance of electric shock) the organism may be pushed beyond the optimal point of the input-output relation. When such loading is removed there is a tendency for reactivity to return to an equilibrium characteristic of a lower energy level.[15] From what is now known it would seem that while these resting levels are rather constant for individuals as measured by static physiological tests, the mechanism by which such equilibrium is maintained differs widely; that is to say, one person may affect equilibration by first order glandular processes while in another emergency mechanisms are called into play frequently as a response to only minor displacing stimuli. In this direction, possibly, lies new light on the ever baffling problems of the 'mental' pathologies. Means are already at hand for assaying the contributions of autonomic and central mechanisms in the mobilization of postural energies, and our understanding of endocrine mechanisms is rapidly increasing. It would seem that thorough study of the role of the postural substrate in response adjustment will well repay the effort which must be expended in the solution of this important problem.

[MS. received November 17, 1937]

[14] *Cf.* W. S. Brown, A note on the psychogalvanic reflex considered in conjunction with estimates of character qualities, *Brit. J. Psychol.*, 1925–26, 16, 130–135; C. W. Darrow and L. Heath, Reaction tendencies relating to personality, *Studies in the Dynamics of Behavior*, Univ. Chicago Press, 1932, 59–261.

[15] G. L. Freeman, Postural accompaniments of the voluntary inhibition of micturition (in press); The effect of inhibited micturition upon interrupted and completed acts of unrelated origin (in press).

PERCEPTION AND INTELLIGENCE: THEIR PHYLOGENETIC RELATION

BY NORMAN M. LOCKE

Hunter College of the City of New York

Introduction

The phenomena of perceptual constancy occur when the physical stimulus and the corresponding psychological reaction are at variance. It can be demonstrated that a physical stimulus can go through a wide range of changes without any change in the experience related to these many stimuli. Regardless of the underlying physical shift the individual reports that he sees the same aspect of the object, hence the term 'constancy.' For example, the physical determinants of the perception of color are the albedo of the object and the illumination in which it stands. If the illumination changes, the physical relations and resulting physical color equation will change. However, the subject reports that the color of the object is unchanged. This constancy holds for other attributes of an object such as its size and shape. Size is a function of the physical dimensions of an object and its distance from the observer. If the distance changes, the size equation changes, but the perceptual experience of the size is relatively stable. The same equations and relations can be constructed for shapes. A constancy of speed has been investigated and measured (**3**). It would seem that every sensory field possesses a constancy although only a few have been studied.

The importance of these constancies seemingly cannot be over-emphasized because basic to every perceptual reaction, regardless of its nature, lies some kind of constancy. Indeed, since one could readily state, as some do, that perception is the alpha and omega of our science, it would be simple to conclude that constancy is the center of psychology.

While engaged for the last few years in experimentation and study of the perceptual constancies, the author noted a striking relationship between the degree of perceptual constancy and the intelligence of an organism. Between these functions one can perceive an inverse correlation the explanation of which unifies and renders meaningful almost all the experimental work so far completed. The results show that as we ascend the phylogenetic scale the organism increases in intelligence and at the same time decreases in degree of perceptual constancy. The author offers the following by way of explanation.

In its interaction with the environment the organism makes use of the psychological functions and mechanisms at its command. Following the usual distinction, let us say that the invertebrates rely primarily upon physiological mechanisms, that the lower vertebrates possess perceptual reactions in addition to the physiological, and that the higher vertebrates have the further factor of intelligence. A recent review and analysis of conditioning (**26**) comes to somewhat the same conclusions.

The lower vertebrate makes his adjustment primarily by way of perceptual ability and the higher vertebrate by way of intelligence. As the given phylum becomes more complex one function increases as the other decreases. If we oversimplify the situation momentarily, we see the lower vertebrate existing in the environment by way of perception and a rudimentary intelligence, and the higher vertebrate in the environment by way of intelligence and an atrophied perception.

To substantiate this thesis we shall present an analysis of many different studies. Since no one experimenter investigated all the phyla in one comparative study we must bear in mind that the conditions of each experiment and the subjects themselves are variables.

Facts of Perceptual Constancy

Fishes.—Burkamp (**7**) tested the tench (*tinca vulgaris, Cyprinides*) for color vision and then investigated color

constancy in this fish. He trained the fishes to feed at colored troughs which hung over the edge of the aquarium, and then changed the illumination by moving the aquarium closer to the window and by closing the shutters. He varied the color of the illumination by passing the light through colored filters. His problem was to discover if fishes continued to go to the original pan to whose color they had been trained despite the physical change in the color equation. There were many opportunities for error, for different colored troughs of slightly different saturation as well as a series of gray troughs were present. Only relative brightnesses are given by the author in some indeterminate scale, but the results are quite clear. Burkamp states (7, p. 163), "The ability of color discrimination of the fish reaches at least the same level of low illumination as that of man. Even in the illumination grade of 5 at which man can no longer recognize colors the trained color is still preferred slightly above the other elementary colors and the black-white colors even though the slightness of the difference and the small number permit no reliable conclusion. At illumination grade 4 there appears to be a preference of the brightness grade 3 and consequently a recognition of 'actual color.' The recognition of the training color is here unmistakable. At the illumination grade 5 we are apparently just on the threshold of color discrimination for the fish whereas for us the threshold has already been passed." In other words, these fishes are far superior to man in the degree of color constancy, having some constancy long after man can no longer distinguish color.

Sumner (27) recently reported that in the pigment migration of the skin of fishes the "response to a given albedo is to a large extent independent of absolute illumination." The skin coloration of the fish, a physiological response, is constant under different conditions of illumination. "Here we have a striking parallel between the physiological responses of a fish and certain well known facts of light perception in ourselves. I refer to the apparent constancy of a given color or shade under widely different conditions of illumination. It is the problem of color-constancy, so-called,

which has received considerable attention from psychologists. Our perception of any one object in the visual field is not an isolated fact. It is influenced more or less by the entire visual field. In the same way, the fish reacts not merely to the light rays reaching its eyes from the background, but reacts, in some way, to the ratio between these rays and the incident light" (**27**, p. 62).

Chickens.—Although there are no direct comparisons of chickens with other groups the observations are worth reporting. Katz and Révész (**18**) trained hens to eat white grains of rice and to reject any grain which had been colored by staining. White grains were then scattered on a white floor and illuminated very strongly with chromatic light. The authors state that the floor appeared at least as highly saturated as the blue or yellow of the colored grains. When placed before the illuminated grains the hens ate them without hesitation even though they had previously rejected grain of the same hue and saturation. Köhler (**21**) reports that hens trained to the whiter of two Zimmermann papers pecked from the whiter paper even though the black paper reflected 7.6 times as much light, on the average. With another hen the ratio was 5.4. Of hens trained to the black, one responded to the black when it was 7.9 times 'lighter' than the white, and the other when the ratio was 10.3. As will be seen below, these ratios are higher than those usually obtained with other vertebrates. In order to express the color constancy response in percentage terms Thouless (**28**) uses the 'index of phenomenal regression,' a logarithmic unit. He states that in Köhler's experiment this coefficient was "greater than .61" (p. 351).

In a study of size constancy, Götz (**14**) found that chicks trained to a large grain of corn chose the larger when its retinal image was one-thirtieth of that of a smaller grain.

Lower primates.—Köhler (**21**) trained a chimpanzee to choose a white paper rather than a black and the animal made this choice when the black paper was illuminated and reflected 7.3 times as much light as the white. A second animal chose the white paper rather than a black which

reflected 4.9 times as much light. Thouless computed the constancy coefficient for the chimpanzees, and, as before, found it to be greater than .61.

Locke (**22**) has reported some comparative figures on four rhesus monkeys and five human adults. Conditions for both animal and human subjects were similar for seven different stimulus conditions. Using a coefficient similar to the index of regression above mentioned he found that in 63 scores there were only two instances of overlapping. The monkeys consistently revealed a higher degree of color constancy than the human beings. As will be shown below, experimental work on human subjects reveals a bimodal distribution in which one group of individuals scores considerably higher than the other. One of Locke's subjects was known from previous experimentation to be in the higher scoring group, and another subject revealed himself in the same category. Even these individuals, the highest in the human group, were far outstripped by the monkeys. The two cases of overlapping occurred with each of these two subjects. A comparative table of the combined scores in seven experimental situations shows these differences:

	Human		Animal
	Low	High	
I	.12	.21	.56
II	.06	.12	.49
III	.07	.08	.24
IV	.06	.15	.33
V	.07	.25	.34
VI	.09	.22	.34
VII	.06	.18	.33

Köhler's report on size constancy (**21**) reveals that chimpanzees chose a larger box to which they had been trained when the retinal image of that box was from 37 to 72 per cent smaller than its comparison box. Locke (**23**) conducted a comparative study of size constancy and found ratios of from .69 to .87 for monkeys. These ratios were no different from those found with human children and adults.

Children.—Brunswik (**4**) found that color constancy in boys and girls increased from a coefficient of .25 in the three-year-old to a peak of .50 to .55 found between the ninth and fifteenth year, from which age it decreased to the score of the six-year-old, .40 to .45. Burzlaff (**8**) argued that the genetic course of color constancy as found by Brunswik was the result of the method used, and in an experiment of his own found that four-year-olds showed complete constancy. Brunswik's reply (**19**) was that Burzlaff had made the task too easy and therefore found high scores.

The same disagreement is encountered in investigations of size constancy. Beyrl (**2**) found a high score, .92, for two-year-olds and a regular increase up to a plateau of complete constancy, .995, at the ten year level and on to the adult. Burzlaff, as before, found complete size constancy in four-year-olds, and Frank (**11**) repeated Burzlaff's comment as to methodology in a criticism of Beyrl. In a repetition of the latter's experiment, Frank found many errors made by three- and four-year-old children whereas with the employment of a different method these errors disappeared.

There is an indication that children have a higher degree of size constancy than do adults. Using children from nine to twelve years of age, Weber and Bicknell (**31**) report higher constancy scores for them than for adults. The authors state that children showed the strongest tendency to size constancy, and men the least. Thouless (**29**) reports that the older of his adult subjects made lower constancy scores than the younger ones. In the study mentioned above, Locke (**23**) found coefficients of from .79 to .90 in four-year-old children. These scores were of the same order as those made by adults. He felt, however, that the experimental situation was too simple to permit group differences to appear.

Since Frank (**10**) states that an eleven-month child responded to a box 6 by 6 centimeters when it was less than one-fifteenth the area of a box 4 by 4, it would seem either that size constancy develops very early in the young child or that we are born with it. A similar study on very young children suggests the answer. Brunswik and Cruikshank (**6**)

found that there is a relatively slight degree of size constancy in infants four to five months of age. At about six months the beginnings of size constancy are noticed. The authors state that "there is a considerable approach to the establishment of size constancy within the perceptual system by the second half of the first year" (**6**, p. 19).

In shape constancy Klimpfinger (**19**) found the same developmental curve as Beyrl did with size and Brunswik with color. The constancy ratio for three-year-olds is .13. This rises to a maximum of .50 at ten years, and as it approaches the adult declines again to .32, the score of young school children.

Adults.—The most relevent studies upon the human adult in the field of color constancy are those which comment upon the bimodal distribution which is usually found with such subjects. Large differences have been found by Brunswik (**5**), Fuchs (**12**), Gelb (**13**), Katz (**17**), and MacLeod (**25**). As an example of the difference in scores, let us take Henneman (**15**, p. 59) who reports the average coefficients of his double grouping as .26 and .62. The same dichotomy of scores obtains for size constancy, as reported by Holaday (**16**), and for shape constancy, as reported by Klimpfinger (**20**). This difference in constancy equation is a real one. Henneman attempted most forcefully to shift the attitude of his subjects, but succeeded only in part and to a slight degree. He states, "The evidence seems to favor the view that personal factors, probably attitudes of perception, are operative in determining the equation. . . ." (**15**, p. 66).

The usual explanation for this divergence in results is in terms of the attitude of the observer. The individual who makes a high constancy score is said to have an object attitude, that of naive observation, and the one who makes a low score is said to have a stimulus attitude, that of analysis. A similar explanation is advanced by Katz in terms of an objective and subjective attitude; by MacLeod in the same terms; Gelb calls the latter the attitude of pure optics; Brunswik speaks of the synthetic and analytic attitudes;

Henneman terms them the object-directed and the photographic attitudes.

From the aspect of the relation of constancy to intelligence, we have a study of Thouless (**29**). A similar study by Locke and Sheehan (**24**) is now in progress. Thouless correlated the results of size, shape, and color (or as he terms it, brightness) constancy with intelligence as measured by the Cattell test and the N.I.I.P. Because of the difficulty raised by the problem of distinguishing between brightness and whiteness, the correlations involving size and shape constancy were considered more reliable. With an average of the size and shape equations of 64 subjects as a measure of perceptual constancy, there was a correlation of − .245, standard error .005, between constancy and intelligence. Thouless states (**29**, p. 234) ". . . the more intelligent subjects tended to see more nearly to the stimulus character of the object." Using 'a better measure of intelligence' the correlation coefficient found with 53 subjects was − .41, SE .01.[1]

Summary and Conclusions

Based on the above material as evidence, it is the contention of the author that the phyla can be ranked in order of degree of perceptual constancy. There are opportunities for direct comparison in some of the quoted studies and these can be supplemented by cross comparisons.[2]

[1] The following studies are not only of interest at this point but also highly suggestive. Klimpfinger (**19**, p. 624) found that peasants have a higher degree of constancy than educated adults. Along the same lines we have a study by Thouless (**30**) who states that he found "a real racial difference in perception" between 20 Indian observers and 49 British in an experiment upon size constancy and shape constancy. The coefficients of size constancy for these groups did not differ, the average being .76, but in shape constancy the average coefficient for the British was .75 and that for the Indians was .86. The author states that "there is a real quantitative difference in perception between the Indian and British students" (p. 336). The higher degree of constancy found with so-called primitive groups is corroborated by a study of Beveridge (**1**). He found that 49 West Africans had an average shape constancy coefficient of .85 compared with an average of .68 for 12 Europeans. In size constancy 44 West Africans had an average constancy index of .88 compared with that of .75 for 8 Europeans.

[2] We have been writing of perceptual constancy as if it were a unitary factor. That we are not alone in this thought is shown by v. Fieandt who states, "In accordance with previous findings perception therefore seems to be a fairly autonomous primi-

The fishes were capable of a color constancy response in illumination so low that man could not perceive color. Chicks score higher than chimpanzees in a similar color constancy situation, and maintain a size constancy response whose extent is in no way approached by man. When we compare human beings with monkeys we find that the monkeys have a much higher degree of color constancy. The so-called primitive scores higher than the average adult and children's constancy, too, is greater than that of the adult; the usual differences stated between these individuals and the average adult include a qualitative difference in intelligence. When adults are tested, a bimodal distribution is found, with a tendency for a higher degree of constancy to be associated with a lower degree of intelligence. The suggestion here is that the individuals forming the upper mode, those with the object attitude, have remained at the peak of the developmental curve of constancy. This situation has come about primarily because perceptual reactions have not been displaced, or overlaid, by intellectual reactions.

An explanation of the perceptual-intellectual relationship in terms of sound psychological concepts has already been indicated. The organism adjusts to its environment in a manner conditioned fundamentally by its structure and function. The lower animals have a higher degree of perceptual constancy than man because they do not have man's substitute equipment. By the same token, man's perceptual mechanism has suffered as his intellectual mechanism has flourished. Perception and intelligence function within the organism as compensatory adjustive systems. As the organism grows more complex there is an increase in intelligence (one sign of increasing complexity) and a correlative decrease in the primitive perceptual mechanism.

We have attempted to confine this paper to a statement of the hypothesis and to present all pertinent evidence rather

tive system in the frame of the total personality, working its own ways of steady, but relatively slow and incomplete establishment of new stimulus relationships as bearers of new sign-representations" (9, p. 495). Statistical evidence is furnished by a tetrad analysis by Thouless who discovered a group factor, "the tendency to phenomenal regression" (22, p. 229).

than to advance a logical argument in favor of the position. We hope that it will prove a contribution to psychology in general, and specifically to the concept of *Psychologie vom Gegenstand her.*

REFERENCES

1. Beveridge, W. M., Racial differences in phenomenal regression, *Brit. J. Psychol.*, 1935, **26**, 59–62.
2. Beyrl, F., Über die Grössenauffassung bei Kindern, *Zsch. Psychol.*, 1926, **100**, 344–371.
3. Brown, J. F., Über gesehene Geschwindigkeiten, *Psychol. Forsch.*, 1928, **10**, 84–101.
4. Brunswik, E., Zur Entwicklung der Albedowahrnehmung, *Zsch. Psychol.*, 1929, **109**, 40–115.
5. ——, Die Zugänglichkeit von Gegenständen für die Wahrnehmung und deren quantitative Bestimmung, *Arch. ges. Psychol.*, 1933, **88**, 377–418.
6. —— and Cruikshank, R. M., Perceptual size-constancy in early infancy, *Proc. Amer. Psychol. Asso.*, 1937, **45**, 18–19.
7. Burkamp, W., Versuche über das Wiedererkennen der Fische, *Zsch. Sinnesphys.*, 1923, **55**, 133–170.
8. Burzlaff, W., Methodologische Beiträge zum Problem der Farbenkonstanz, *Zsch. Psychol.*, 1931, **119**, 177–235.
9. v. Fieandt, K., Dressurversuche an der Farbenwahrnehmung, *Arch. ges. Psychol.*, 1936, **96**, 467–495.
10. Frank, H., Untersuchung über Sehgrössenkonstanz bei Kindern, *Psychol. Forsch.*, 1926, **7**, 137–145.
11. ——, Die Sehgrössenkonstanz bei Kindern, *Psychol. Forsch.*, 1928, **10**, 102–106.
12. Fuchs, W., Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *Zsch. Psychol.*, 1923, **92**, 249–325.
13. Gelb, A., Die Farbenkonstanz der Sehdinge, *Handb. norm. pathol. Physiol.*, 1929, XII/1, Receptionsorgane II, Photoreceptoren, 594–677.
14. Götz, W., Experimentelle Untersuchungen zum Problem der Sehgrössenkonstanz beim Haushuhn, *Zsch. Psychol.*, 1926, **99**, 247–260.
15. Henneman, R. H., A photometric study of the perception of object color, *Arch. Psychol.*, 1935, **179**, pp. 88.
16. Holaday, B. E., Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. ges. Psychol.*, 1933, **88**, 419–486.
17. Katz, D., Der Aufbau der Farbwelt, *Zsch. Psychol.*, Ergänzungsband, 1930, **7**, 2 aufl., pp. 484.
18. —— and Révész, G., Experimentelle Studien zur vergleichenden Psychologie (Versuch mit Hühnern), *Zsch. angew. Psychol.*, 1921, **18**, 307–320.
19. Klimpfinger, S., Die Entwicklung der Gestaltkonstanz vom Kind zum Erwachsenen, *Arch. ges. Psychol.*, 1933, **88**, 599–628.
20. ——, Über den Einfluss von intentionaler Einstellung und Übung auf die Gestaltkonstanz, *Arch. ges. Psychol.*, 1933, **88**, 551–598.
21. Köhler, W., Optische Untersuchungen am Schimpansen und am Haushuhn, *Abh. preuss. Akad. Wiss., Math. Phys. Kl.*, 1915, **3**, 18–70.

22. LOCKE, N. M., Color constancy in the rhesus monkey and in man, *Arch. Psychol.*, 1935, **193**, pp. 38.
23. ——, A comparative study of size constancy, *J. Genet. Psychol.*, 1937, **51**, 255–265.
24. —— AND SHEEHAN, M. R., A correlational investigation of perceptual constancy and intelligence (in progress).
25. MACLEOD, R. B., An experimental investigation of brightness constancy, *Arch. Psychol.*, 1932, **135**, pp. 102.
26. RAZRAN, G. H. S., Conditioned responses: an experimental study and a theoretical analysis, *Arch. Psychol.*, 1935, **191**, pp. 124.
27. SUMNER, F. B., Changeable coloration, *Sci. Mon.*, 1937, July, 60–65.
28. THOULESS, R. H., Phenomenal regression to the real object. I., *Brit. J. Psychol.*, 1931, **21**, 339–359.
29. ——, Individual differences in phenomenal regression, *Brit. J. Psychol.*, 1931, **22**, 216–241.
30. ——, A racial difference in perception, *J. Soc. Psychol.*, 1933, **4**, 330–339.
31. WEBER, C. O. AND BICKNELL, N., The size-constancy phenomenon in stereoscopic space, *Amer. J. Psychol.*, 1935, **47**, 436–448.

[MS. received December 3, 1937]

PSYCHOLOGICAL REVIEW PUBLICATIONS

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Herbert S. Langfeld, Editor PSYCHOLOGICAL REVIEW,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Samuel W. Fernberger, Editor JOURNAL OF EXPERIMENTAL PSYCHOLOGY,
University of Pennsylvania, Philadelphia, Pa.

Contributions intended for the Psychological Monographs should be addressed to

Professor John F. Dashiell, Editor PSYCHOLOGICAL MONOGRAPHS,
University of North Carolina, Chapel Hill, N. C.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor John A. McGeoch, Editor PSYCHOLOGICAL BULLETIN,
Wesleyan University, Middletown, Conn.

All business communications should be addressed to

Psychological Review Company, Ohio State University, Columbus, Ohio

---

THE PSYCHOLOGICAL REVIEW

is indexed in the

International Index to Periodicals

to be found in most public and
college libraries

COLOR-MUSIC

By THEODORE F. KARWOSKI
and HENRY S. ODBERT
Dartmouth College

A preliminary survey of 274 college students showed that 60 per cent of them associated color with music. Thirty of these, who showed the most complex synesthesia, were studied intensively over a long enough period to show a basic consistency in the association. The theoretical implications of the findings are discussed.

To the exponents of color-music as an art form these findings will have a special significance.

Psychological Monographs — **Whole No. 222**
Sixty Pages — **One Dollar**

THE PSYCHOLOGICAL REVIEW COMPANY
THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO